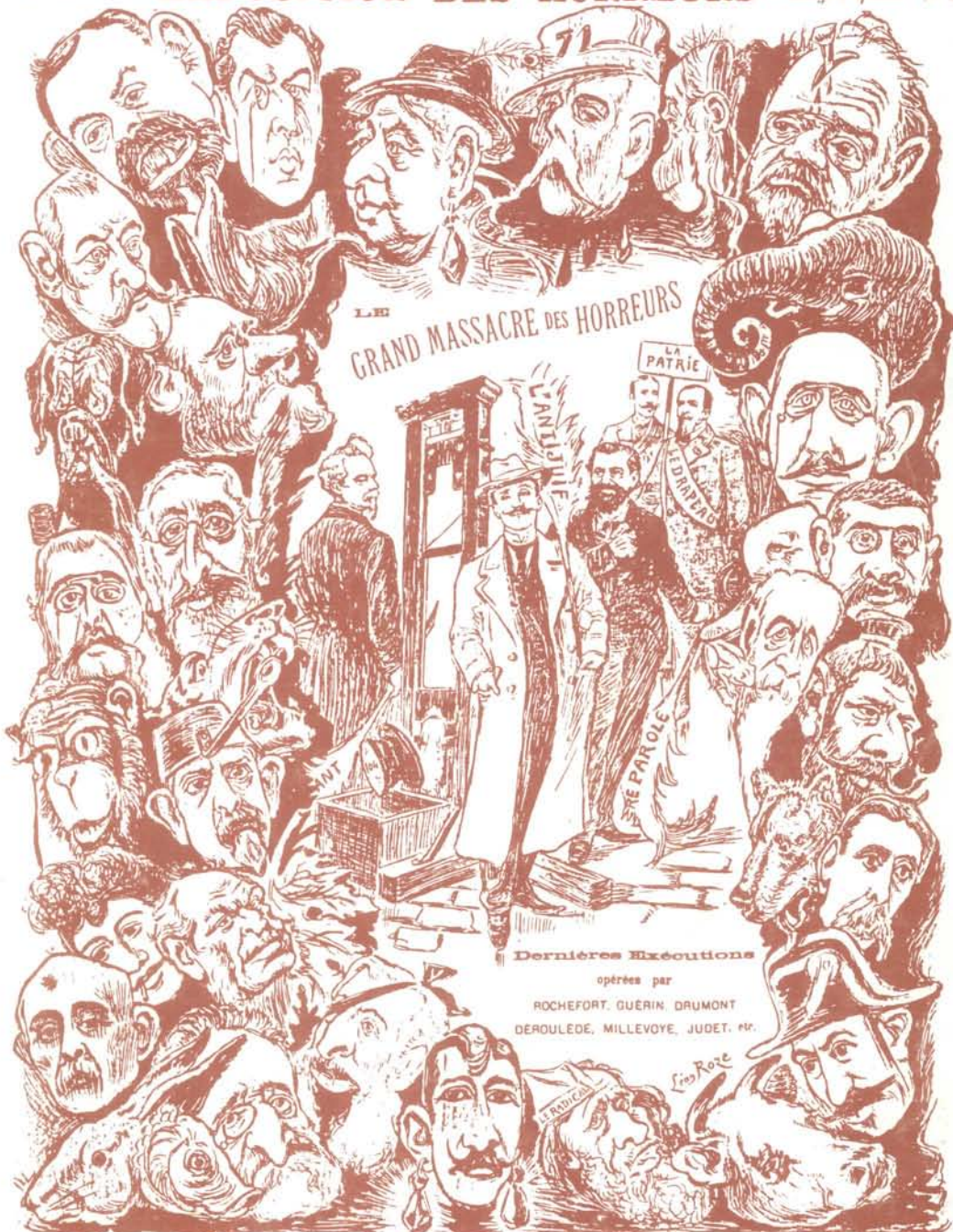


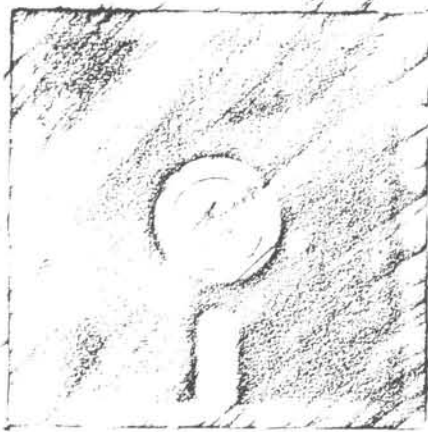
The American Historical Review

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In This Issue

In his presidential address, **William E. Leuchtenburg** explores the tension between the historian's actions as a citizen committed to political causes and the equally important aspiration of a scholar to observe and understand the world from what Leuchtenburg describes as a detached and neutral point of view.

Linda Gordon finds recognizably gendered views of the welfare state among social reformers in the United States. Her examination of the ideas behind the Social Security Act of 1935 shows the social insurance programs (Old Age Insurance and Unemployment Insurance) to have been designed principally by men with the goal of eliminating poverty. Public assistance programs, such as Aid to Dependent Children, designed to eliminate "pauperism" (a condition in which the work ethic is absent and handouts expected), were influenced by women reformers and included a commitment to casework. Though the men and women differed over the importance of casework, both supported the concept of a family wage and its implication that men ought to be the primary breadwinners in the family and women housewives and mothers.

Nancy Fitch challenges recent studies that discount the importance of the Dreyfus Affair and anti-Semitism in rural France. She sees the countryside as well-integrated into the national economic, political, and cultural life. Printers of low-cost, small-format items flooded French villages with a wide array of anti-Semitic newspapers, images, and trinkets. The purveyors of this material reworked the established stereotype of the "usurious Jew" to represent a variety of social dangers, and agrarian elites exploited the fears thus engendered to win support for authoritarian and anti-socialist politics. Their success, according to Fitch, was great enough to prompt a modification of socialist positions on the Jewish question, parliamentary participation, and land reform.

Mercedes Vilanova asks for greater attention to the role of illiterates in politics. Her study of voting patterns in Barcelona during the Spanish Republic shows that differences in literacy were more important than differences in sex in determining electoral behavior. Her data also reveal as fallacious the common belief that anarchist campaigns in favor of abstaining from the vote undermined democracy in Spain. Vilanova derives some tentative lessons about the cultural and social isolation of illiterates on the basis of oral interviews with a few illiterate workers who survived from the 1930s into the present day.

Two review essays follow. In the first, **Patricia Nelson Limerick** leads us on a tour of America's Far West, the history of the Hawaiian Islands as portrayed in four recent works focusing on issues of demographic collapse, missionary women, Hawaiian women, and fascinating questions of authorial voice and the anthropology of writing history itself.

Michael Kazin looks at recent studies of the Ku Klux Klan of the 1920s and the New Right of the 1960s and 1970s. Scholars have begun to identify the American Right with a populist outlook and to apply to grass-roots conservatives the same methods and interest that historians have for some time been bringing to studies of American workers, minorities, and the Left. Kazin welcomes this new approach but urges scholars of right-wing populism to go beyond their revision of the former view of these people as kooks and extremists to an exploration of the variety of meanings of "traditional values," the racist implications of conservative ideology, and comparisons of American conservatism with popular right-wing movements in other countries.

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The Historian and the Public Realm

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The Historian and the Public Realm

WILLIAM E. LEUCHTENBURG

OVER THE PAST CENTURY, no question has more polarized our profession than the dispute over what is the appropriate relationship of history to the public realm. Generation after generation, a substantial corps of scholars has insisted that historians should concentrate on contributing to the solution of contemporary problems. Indeed, the conviction that history should cater to the present goes back to the earliest days of this association, when it was voiced by the very first presidents of the AHA, including Andrew Dickson White and Charles Kendall Adams. A generation later, one of C. Vann Woodward's patrons declared, "If Dr. Johnson were alive today, he would say it was [pure] research which is the last refuge of the scoundrel." On the other hand, more than half a century ago Robert Livingston Schuyler celebrated "the usefulness of useless history," and more recently Theodore S. Hamerow, confronted by "that troublesome question with which historians are constantly assailed: 'What is the use of history?'" replied, "The answer is that history is of no use; it simply is." On only one point have the two sides agreed—that their positions are irreconcilable.¹

The belief that history should address the urgencies of the day has taken two forms, the first of which is the claim that current needs should be privileged in the writing of history. That notion found classic expression in 1907 when James Harvey Robinson and Charles A. Beard boasted that they had "consistently subordinated the past to the present" in keeping with their "ever-conscious aim to enable the reader to catch up with his own times; to read intelligently the foreign news in the morning paper; to know what was the attitude of Leo XIII toward the social democrats even if he has forgotten that of Innocent III toward the Albigenses."²

This presidential address to the American Historical Association was delivered in Chicago on December 28, 1991. The author is grateful to Allan Brandt, Alan Brinkley, Jean Anne Leuchtenburg, and Edwin M. Yoder, Jr., for insightful comments and is appreciative of materials sent to him by Leon Fink, Otis L. Graham, Jr., Michael Hunt, Robert Kelley, J. Morgan Kousser, David E. Kyvig, Peyton McCrary, Page Putnam Miller, and Raymond Wolters.

¹ Herman Ausubel, *Historians and Their Craft: A Study of the Presidential Addresses of the American Historical Association, 1884–1945* (rpt. edn., New York, 1965), 24; John Herbert Roper, *C. Vann Woodward, Southerner* (Athens, Ga., 1987), 4, quoting the dean of the graduate school at Louisiana State University, Charles Wooten Pipkin; Robert Livingston Schuyler, "The Usefulness of Useless History," *Political Science Quarterly*, 56 (March 1941): 23–37; Theodore S. Hamerow, *Reflections on History and Historians* (Madison, Wis., 1987), 33. For the wide divergence of views on the proper role for the historian, see Norman Graebner, "The State of Diplomatic History," *Society for Historians of American Foreign Relations Newsletter* (March 1973): 2–12.

² James Harvey Robinson and Charles A. Beard, *The Development of Modern Europe: An Introduction to the Study of Modern Europe*, 2 vols. (Boston, 1907–08), 1: iii. For especially ardent espousal of history

The emphasis on applicable history has also taken a second form: advocacy of direct attempts by historians to shape public policy. When Herbert Baxter Adams secured for the American Historical Association the unusual recognition from Congress of a federal charter, he anticipated, in John Higham's words, that he had "opened a channel through which the aristocracy of culture might, in historical matters, exert a vigorous, uplifting influence on national politics." In the Progressive Era, that archetypal figure, Charles A. Beard, who had worked at Hull House when he was a college student and had helped establish a workingman's college at Oxford in his graduate student days, continued, while professor at Columbia, to participate in the activities of civic reform groups such as the National Municipal League and campaigned for a Socialist congressman. Subsequently, he served as an adviser to governments in the Balkans and in Japan. By the time the United States intervened in World War I, it seemed altogether natural for John Franklin Jameson to organize a National Board for Historical Research, put together lectures on history for delivery at army training camps, and place the *American Historical Review* in the service of the government by seeking articles establishing German war guilt.³

World War II opened further opportunities for historians. Even before Pearl Harbor, the federal government created a board to analyze foreign intelligence under the diplomatic historian, James Phinney Baxter III, president of Williams College, and Baxter, in turn, appointed the Harvard historian, William L. Langer, to direct research. Out of those beginnings came a new agency, the Office of Strategic Services, with Langer as chief of Research and Analysis, an endeavor that involved some of the most prominent senior historians in the country, including Hajo Halborn and my former teacher, Franz Neumann, as well as a brilliant galaxy of younger men including Franklin Ford, H. Stuart Hughes, Carl Schorske, and Robert Wolff.⁴

When, less than a decade later, the landmark case of *Brown vs. Board of Education* was being considered by the Supreme Court, the Justices, John Hope Franklin has recalled, raised a number of "searching and quite difficult questions [that] sent legal counsel scurrying not to the history books but to the historians!" In numerous papers prepared for the attorneys, in seminars and conferences conducted for the staff of the NAACP Legal Defense Fund, and in more informal ways, Franklin, C. Vann Woodward, and other historians made it possible for counsel for black pupils to parry the argument that the framers of the Fourteenth Amendment did not intend it to empower the national government to desegregate schools. Although the court's decision in *Brown* could not be shown to have turned on the evidence adduced by the historians, it could be said, as Franklin

geared to contemporary issues, see Robert S. Lynd, *Knowledge for What? The Place of Social Science in American Culture* (Princeton, N.J., 1939), 133; and Howard Zinn, *The Politics of History*, 2d edn. (Urbana, Ill., 1990), 309.

³ John Higham, *History: Professional Scholarship in America* (Baltimore, Md., 1989), 14, 124. Morey Rothberg discusses Jameson in a forthcoming edition of Jameson's work for which I have written an introduction. For the role of scholars in the World War I era, see Lawrence E. Gelfand, *The Inquiry* (New Haven, Conn., 1963).

⁴ William L. Langer, *In and Out of the Ivory Tower: The Autobiography of William L. Langer* (New York, 1977), 180–93.

observes, that historians “had answered the call to participate in an important public policy issue; and it would seem that their participation had been effective.”⁵

In the nearly four decades since Franklin and his associates helped to bring about the demise of Jim Crow, historians have made their mark in the public realm in countless ways. Arthur Schlesinger, Jr., served under President Kennedy and Eric Goldman in the Johnson White House. During the Vietnam War, petitions published in the *New York Times* in 1967 with the message “Mr. President: Stop the Bombing!” bore the signatures of 184 historians. Robert Kelley, J. Morgan Kousser, Peyton McCrary, Allan Lichtman, and several others have been employed as expert witnesses in litigation ranging from environmental policy to voting rights, and historians recently assisted in the brief of a significant abortion suit, *William L. Webster, et al. vs. Reproductive Health Services, et al.* After the Supreme Court in a 1980 ruling announced that it required historical evidence of discriminatory intent in voting rights cases, the Justice Department, the NAACP Legal Defense Fund, and other plaintiffs “had little choice,” as Kousser noted, “but to call in the historians.” In fact, it has been said that “some cases have been decided primarily because the courts have placed credence in testimony by historians.”⁶

The past generation has seen, too, the burgeoning field of public history come into its own. Large numbers of historians have found jobs in government agencies, national and local, as well as in the private sector, and the United States Senate, the House of Representatives, and the Federal Judicial Center have set up historical offices that have proven to be of inestimable value both to those institutions and to historians. With startling swiftness, the field grew large enough to make possible the birth of a National Council on Public History, two new professional journals, and, at the University of California at Santa Barbara, an undergraduate major in the history of public policy. Implicit in these developments has been an assumption about what role the historical guild should perform. Peter N. Stearns and Joel A. Tarr, directors of a program in “applied history” at Carnegie-Mellon University, have lauded historians who, by “applying historical thinking to the making of public policy,” thereby “depart from the discipline’s narcissism.”⁷

⁵ John Hope Franklin, “The Historian and the Public Policy,” in Franklin, *Race and History: Selected Essays, 1938–1988* (Baton Rouge, La., 1989), 312–14.

⁶ Everett Carl Ladd, Jr., “American University Teachers and Opposition to the Vietnam War,” *Minerva*, 8 (October 1970): 545; Robert Kelley, *Battling the Inland Sea: American Political Culture, Public Policy and the Sacramento Valley, 1850–1986* (Berkeley, Calif., 1989), xviii; J. Morgan Kousser, “Are Expert Witnesses Whores? Reflections on Objectivity in Scholarship and Expert Witnessing,” *Public Historian*, 6 (Winter 1984): 11; *City of Mobile v. Bolden*, 446 U.S. 55 (1980); Peyton McCrary and J. Gerald Hebert, “Keeping the Courts Honest: The Role of Historians as Expert Witnesses in Southern Voting Rights Cases,” *Southern University Law Review*, 16 (Spring 1989): 101, citing decisions such as *Bolden v. City of Mobile*, 542 F. Supp. 1050 (S. D. Ala. 1982); Peyton McCrary, “Racially Polarized Voting in the South: Quantitative Evidence from the Courtroom,” *Social Science History*, 14 (Winter 1990): 507–31; Chandler Davidson, ed., *Minority Vote Dilution* (Washington, D.C., 1984).

⁷ Kelley, *Battling the Inland Sea*, 339–40; Robert Kelley, “Public History: Its Origins, Nature, and Prospects,” *Public Historian*, 1 (Fall 1978): 21–22; Barbara J. Howe and Emory L. Kemp, eds., *Public History: An Introduction* (Malabar, Fla., 1986); Susan Porter Benson, Stephen Brier, and Roy Rosenzweig, eds., *Presenting the Past: Essays on History and the Public* (Philadelphia, 1986); David F. Trask, “The State of Public History in the Washington Area,” *Public Historian*, 1 (Fall 1978): 37–41; *New York Times*, June 7, 1980. “Public history” is often treated as a modern-day upstart, but as the

Vigorous though these manifestations of public activity have been, they have run up against a considerably stronger contrary emphasis, given its most unequivocal expression by Julien Benda. In his widely noted 1927 volume, *La trahison des clercs* (*The Treason of the Intellectuals*), Benda deplored "contempt for the man who shuts himself up with art or science and takes no interest in the passions of the State." The modern intellectual, he complained, was "violently on the side of Michelangelo crying shame upon Leonardo da Vinci for his indifference to the misfortunes of Florence, and against the master of the Last Supper when he replied that indeed the study of beauty occupied his whole heart." Benda applauded the example of Goethe, who said, "Let us leave politics to diplomats and the soldiers," and who in *Dichtung und Wahrheit* reported his response and that of his friends to the French Revolution: "In our circle, we took no notice of news and newspapers; our object was to know Man; as for men, we left them to do as they chose." Benda demonstrated that he took that counsel literally when in 1941, at a time when Nazi troops were occupying France, he wrote André Gide, "L'inactuel, mon vrai domaine." As the German scholar Wolf Lepenies has commented, "For Benda, avoiding being up to date was, for the intellectual, an important virtue which had almost been destroyed by the Dreyfus Affair and its aftermath. The treason of the intellectuals consisted mainly in their attempt to enter politics and thereby exert an influence on the issues of the day."⁸

Although few would go as far as Benda, many historians share his discomfort with the effort to be timely. In postwar America, the progressive school of history associated with Charles Beard fell out of favor, and, as John Higham remarked, "the label *present-minded* now loomed up as an epithet." Oscar Handlin warned against promising that history "would equip citizens with the nostrums to dissolve current and future problems," for "other, more flexible departments of knowledge could always outbid it in a marketplace geared to relevance." Handlin extolled instead the example of "clerks in the Dark Ages who . . . by retiring from an alien world to a hidden monastic refuge" managed to "maintain a true record . . . [that] informed the future of what had transpired in their day."⁹

Historians surveying the state of the discipline have reported pervasive sentiment for disengagement. In 1964, J. H. Plumb observed that "fewer and fewer historians believe that their art has any social purpose; any function as a coordinator of human endeavour or human thought." Considerably more emphatic was Theodore Hamerow. In a book published just four years ago, he found "growing recognition that scholarship can offer no guarantees for the solution of social problems," and that "we have had to recognize that history in this sense is 'irrelevant.'" He wrote of historians today:

chairman of the National Council on Public History pointed out, "History has been practiced outside the academy for generations." Michael C. Scardaville, "Looking Backward Toward the Future: An Assessment of the Public History Movement," *Public Historian*, 9 (Fall 1987): 37.

⁸ Julien Benda, *The Treason of the Intellectuals*, Richard Aldington, trans. (New York, 1928), 46–47, 81, French edn. appeared the year before; National Humanities Center, *Newsletter*, 12 (Fall–Winter 1990–91): 8.

⁹ Higham, *History*, 132; Oscar Handlin, *Truth in History* (Cambridge, Mass., 1979), 415.

Now, after all the bold ventures and exciting experiments in historical investigation of the last generation, they are less certain than ever of the importance of history for the education of the citizen, the conduct of the government, or the guidance of the community. These doubts are so profound and persistent as to suggest a grave crisis, the gravest perhaps since the emergence of history as an organized profession about a hundred years ago.¹⁰

When I contemplate this predication of "crisis," I do so, inevitably, from the perspective of a historian who has been engrossed in the public realm for fully half a century. So compelling did political concerns seem to me when I was young that for a time I abandoned graduate studies to pursue them. In the years I was in and out of graduate school, I served as Queens County Director, then State Youth Director, of the Liberal Party; as Assistant Editor of publications of the American Labor Conference on International Affairs, designed to provide material on foreign affairs to the labor press; as New England Field Representative for a civil rights lobby, the National Council for a Permanent Fair Employment Practices Commission headed by A. Philip Randolph; as upstate New York petition canvasser for Governor Herbert Lehman and Senator James Mead; as National Executive Secretary of Students for Democratic Action; and as Rocky Mountain organizer for Americans for Democratic Action. Subsequently, I became ADA's representative on the staff of Richard Bolling in Kansas City in his first campaign for Congress in 1948 and then State Director of its Massachusetts chapter, where I also functioned as speech writer for the governor, testified before legislative panels, chaired the United Labor Committee, and organized Boston's first citywide committee against racial discrimination.

When I drifted back into the Ph.D. program at Columbia, I did so on the understanding that I could write a dissertation that was congruent with my political interests, and when, simultaneously, I moved from my ADA office on Beacon Street to a teaching job in Northampton, it was not in history but in political science. While teaching at Smith, I spent summers on the staff of a CIO union giving courses in current affairs and political action to factory workers, and was appointed campaign manager for a union leader in Holyoke who was Democratic nominee for Congress. (I might add that so sharply honed were my political skills by then that Anna Sullivan became the worst defeated candidate in the history of western Massachusetts.)

My teaching and writing have dovetailed with my political interests. At Harvard, where I held my first college teaching job in history, I invented a course called "The Progressive Tradition in American Politics," and I have written almost exclusively in the field of recent American history so that I could keep one foot in the present, where I continued to be politically active. In the process of moving from Harvard to Columbia in the summer of 1952, I took a post as Western Field Representative for a presidential candidate in Utah and Wyoming, and at the 1952 Democratic National Convention in Chicago worked, along with the pollster Louis Harris, as "delegate analyst" in charge of estimating how each state would vote on the first and succeeding ballots. Shortly after arriving at Columbia, I was elected Democratic county committeeman in Westchester

¹⁰ J. H. Plumb, "The Historian's Dilemma," in Plumb, ed., *Crisis in the Humanities* (Baltimore, Md., 1964), 25-26; Hamerow, *Reflections*, 12, 3.

County, and I consumed my first sabbatical as New York State chairman of Americans for Democratic Action.

I can only suggest the range of public activities in which I have been engaged since that time. I spent several November nights writing presidential election analysis for NBC, first for Chet Huntley and David Brinkley, then for John Chancellor; took part with other historians on the final day of the Montgomery march with Martin Luther King; sued Richard Nixon to deny him the right to destroy the Watergate tapes, and, again in league with other scholars and journalists, sued Henry Kissinger to prevent him from sequestering his transcripts of official telephone conversations; commuted to Washington for two years as the AHA's representative on the National Study Commission on Records and Documents of Federal Officials chaired by the former Attorney General of the United States, Herbert Brownell; gave a featured talk to the Democratic Leadership Council on the vitality of liberalism (a message I very much doubt they wanted to hear); testified before the Senate Judiciary Committee against confirmation of Robert H. Bork to the Supreme Court; served on an Advisory Committee on Oral History headed by Robert F. Kennedy; participated in any number of secondary school programs and on the Bradley Commission on History in Schools as well as its successor, the National Council for History Education; honored the memory of Eleanor Roosevelt at ceremonies at Vassar College and elsewhere; entered into a discussion on whether the presidency was in crisis with Jimmy Carter at the Wilson Center; gave literally thousands of newspaper interviews—for example, to the *Baltimore Sun* about Reagan's place in history; to the *Detroit Free Press* on morality and politics; and to the *Wall Street Journal* on the relation of private behavior to public performance in the White House; was consultant for a good number of documentary films, including *The Civil War*; spoke in the French Senate at the centennial of France's gift of the Statue of Liberty; joined with William Chafe, John Hope Franklin, and Anne Firor Scott in raising many thousands of dollars from historians for Harvey Gantt's campaign to unseat Jesse Helms; was heard on scores of radio programs in cities in this country such as Charleston and Cincinnati and abroad in cities from Vancouver to Melbourne; appeared on a great many television programs including CBS Evening News with Dan Rather, ABC Nightline, an NBC special, Walter Cronkite's CBS Reports, BBC, and Norwegian Television; worked with Bill Moyers as a member of the CBS team covering the 1985 inauguration and with Paul Duke of Washington Week in Review on the PBS team covering the 1989 inauguration; and reminded members of Congress at the home of my former student, Congressman Stephen J. Solarz, of the shameful failure of the American government headed by Franklin D. Roosevelt to provide a haven for European Jews, millions of whom would be murdered by the Hitler government in the Holocaust.¹¹

In the past months alone, I talked to a gathering of United States Senators, was

¹¹ William E. Leuchtenburg, *Flood Control Politics: The Connecticut River Valley Problem, 1927–1950* (Cambridge, Mass., 1953); William E. Leuchtenburg, "The Montgomery March," *American Heritage*, 40 (December 1989): 66–68; *New York Times*, February 11, 1977; Woodrow Wilson International Center for Scholars, *Jimmy Carter on the Presidency: A Wilson Center Conversation, March 5, 1984* (Washington, D.C., 1984).

interviewed by the *New York Times*, the Associated Press, the *Atlanta Constitution*, the *Los Angeles Times*, the *New York Daily News*, the *Kansas City Star*, the *Washington Post*, and *USA Today*, spoke on National Public Radio's All Things Considered, was consulted by one of Mario Cuomo's advisers on how FDR managed to run for president while in the midst of a budget crisis in Albany, sent memos to Ken Burns for his forthcoming documentary film on the history of baseball, published an article in the popular history journal, *American Heritage*, consented to serve on a National Coordinating Council on setting standards for the teaching of history K through 12, and promised to give the keynote address at the next annual conference of the National Council on Public History.¹²

How then does someone with this background respond to the assertion that writing history is justifiable wholly apart from any utility to the public realm? I unequivocally agree. When Professor Hamerow states that "the importance of history is essentially intrinsic; it lies in the interest in the past which human beings instinctively feel as part of their humanity," and that "the life of the community cannot continue without it," I readily concur. For millennia, people have found history indispensable to comprehending who they are, and I anticipate that they always will. "A people without history," say the Lakota tribe, "is like wind upon the buffalo grass." Moreover, insofar as history is an art form, which the best historical writing surely is, it no more needs justification by good works than does a sonnet or a sonata.¹³

Those who insist that history is worthwhile only when it offers solutions to current problems reveal a hostility to the very nature of the historical enterprise. In the famous passage obliterating the Albigenses in the Robinson and Beard book, the fundamental objection was, as the philosopher Morton White has pointed out, that "the medievalist was interested in explaining medieval events when he should have been trying to illuminate modern events," a judgment that suggests a passion for contemporaneity run amok. It would be hard to imagine anything more ill-advised than for all historians, including those in medieval history, to tailor their research to the morning's headlines. The humanities, asserted the philosopher Charles Frankel, "have usually been at their best and most vital . . . when they have had a sense of engagement with issues of public concern," and he demonstrated that belief by taking leave from Columbia to become Assistant Secretary of State for Educational and Cultural Affairs. Yet Frankel also declared, "Scholarship cannot and should not be shackled to problem solving. It must be free to follow crooked paths to unexpected conclusions."¹⁴

Despite a lifetime of civic engagement, I also find totally repugnant any effort to politicize this organization or to impose favored orthodoxies on the classroom. Indeed, it is precisely those who have been most involved in public affairs who

¹² *Senate History* (Fall 1991): 1; William E. Leuchtenburg, "The Conversion of Harry Truman," *American Heritage*, 42 (November 1991): 55–68.

¹³ Hamerow, *Reflections*, 12, 33. Peter N. Carroll, *Keeping Time: Memory, Nostalgia, and the Art of History* (Athens, Ga., 1990), 178, quoted in David E. Kyvig, "Public or Perish: Thoughts on Historians' Responsibilities," *Public Historian*, 13 (Fall 1991): 13. See, too, Carl N. Degler, "Remaking American History," *Journal of American History*, 67 (June 1980): 23.

¹⁴ Morton G. White, *Social Thought in America: The Revolt against Formalism* (New York, 1952), 50; *New York Times*, July 2, 1978.

have been most resistant to such attempts. Few of us have a longer record of political participation than Arthur Schlesinger, whom I first met at the founding convention of Americans for Democratic Action more than forty years ago, but it was he and Kenneth Jackson, the pioneering head both of the Bradley Commission and of the National Council for History Education, who offered an eloquent remonstrance against New York's unwise plan to warp the curriculum. At Columbia in 1968, those who were most vocal against the assault on the university were the historians and political scientists who had been working for two years against the Vietnam War under the leadership of Fritz Stern, a circumstance that accounts for our being known as "the Stern gang." President Reagan's intervention in Nicaragua appalled me, but when, while I was president of the Organization of American Historians, a resolution was introduced to put the OAH on record in opposition, I cast the lone vote on the executive board against it, because I thought it an abuse of our authority. And although in the past year I was outspoken in denouncing Bush's actions in the Persian Gulf, I also insisted that this association should not take a stand, for I would no more want to inflict my views on others than have views inflicted on me, nor would I wish to see us torn apart by factional fights over such issues.

I hope never again to witness a night like the one at the AHA convention twenty-two years ago when historians grappled with one another for control of the microphone during the bitter debate over resolutions on Vietnam and civil rights with Vann Woodward, in the words of the *New Republic*, "presid[ing] over the cacophony with the puzzled air of a kindly Southern judge at a hearing for psychiatric commitment."¹⁵ One memory of that turbulent night that sticks in my mind is of the man standing next to me in the crowded hall: my former colleague, Orest Ranum, whose years of research notes were deliberately incinerated in the Columbia uprising of 1968.

I saw all too painfully at Columbia that year, when I was a member of the faculty committee that ran the university after the chaos of the spring, and in later years what "politicization" could mean: the paralysis of a great university, the trashing of classrooms by hit-and-run marauders, and physical assaults on professors. Elsewhere, the consequences were sometimes worse, resulting even in death.

Historians long involved in the public realm have also been among the most forthright in underscoring the perils of such involvement for scholarship. When I first arrived at Columbia to teach in the fall of 1952, Richard Hofstadter was seeking to deny the president of the university, Dwight Eisenhower, the opportunity to become president of the United States, and years later we worked side by side in "the Stern gang." Nonetheless, Hofstadter warned, "The activist historian who thinks he is deriving his policy from his history may in fact be deriving his history from his policy, and may be driven to commit the cardinal sin of the historical writer: he may lose his respect for the integrity, the independence, the pastness, of the past."¹⁶

¹⁵ C. Vann Woodward, *The Future of the Past* (New York, 1989), 4.

¹⁶ Richard Hofstadter, *The Progressive Historians: Turner, Beard, Parrington* (New York, 1968), 464–65. For examples of how political commitments may affect scholarly writing, see Harry Elmer Barnes, *In Quest of Truth and Justice: De-Bunking the War Guilt Myth* (Chicago, 1928); Conyers Read,

In his searching analysis of the progressive historians, Hofstadter wrote of the most prominent of them: "Today [Charles A.] Beard's reputation stands like an imposing ruin in the landscape of American historiography. What was once the grandest house in the province is now a ravaged survival." What had gone wrong? Beard had risked too much on "a daring gamble," Hofstadter maintained, for "he had never been content with the role of the historian or the academic alone; he had always hoped to be politically relevant, had always aspired to become a public force . . . And yet any man who makes written commitments year after year on difficult public questions will live to find some of his views evanescent and embarrassing." Moreover, "Beard took a further and more gratuitous risk; he finally geared his reputation as a historian so closely to his political interests and passions that the two were bound to share the same fate," and that fate was disaster. "In proposing not just to draw general moral lessons about the direction and meaning of history but to forge specific recommendations for policy upon which he believed the life and death of American democracy depended," Beard, Hofstadter concluded, "became our supreme tragic example of the activist mind in history."¹⁷

Scholars with the greatest experience in public affairs have been even more emphatic about the difficulties historians encounter when they seek to shape policy. Despite his close identification with such efforts, Arthur Schlesinger has asserted:

History . . . can answer questions, after a fashion, at long range. It cannot answer questions with confidence or certainty at short range. Alas, policy makers are rarely interested in the long run—"in the long run," as Keynes used to say, "we are all dead"—and the questions they put to history are thus most often the questions which history is least qualified to answer.

Far from offering a short cut to clairvoyance, history teaches us that the future is full of surprises and outwits all our certitudes.

Confronted by Ernest May's proposition that "if history is to be better used in government, nothing is more important than that professional historians discover means of addressing directly, succinctly, and promptly the needs of people who govern," Schlesinger retorted, "It may well be more important for professional

"The Social Responsibilities of the Historian," *AHR*, 55 (January 1950): 283–84; Michael A. Bernstein, "American Economic Expertise from the Great War to the Cold War: Some Initial Observations," *Journal of Economic History*, 50 (June 1990): 408. C. Vann Woodward has confessed that in his eagerness to develop the theme of *The Strange Career of Jim Crow* he overlooked the fact that, prior to segregation, blacks had no public space at all. "The oversight illustrates the dangers of allowing present-day issues to shape or define historical investigation," he has written. Woodward, *Thinking Back: The Perils of Writing History* (Baton Rouge, La., 1986), 96–97. One writer had no qualms about stating that "the public historian . . . may have to bend the findings to the whims of the project design that the client has in mind." Lawrence De Graaf, "Summary: An Academic Perspective," *Public Historian*, 2 (Spring 1980): 69. Public historians, however, have given considerably more sustained attention to ethical problems than have academic historians. See, for example, Ronald C. Tobey, "The Public Historian as Advocate: Is Special Attention to Professional Ethics Necessary?" *Public Historian*, 8 (Winter 1986): 21–30.

¹⁷ Hofstadter, *Progressive Historians*, 344–45, 464.

historians to write the best professional history they can and trust to the multiplier effect."¹⁸

The conviction that a greater role for scholars in the State would be advantageous rests on the assumption that they are more farsighted and more humane than those in power. We would do well to remember, though, that it was not so long ago that most members of this association sanctioned the institutions of white supremacy that emerged out of Reconstruction and an even shorter time ago that not a few historians were apologists for Stalin's despotic regime, even though it stifled freedom of expression, sent dissenters to vile prison camps, and was responsible for millions of deaths. In the past generation, we have had reason enough to know that the country's fate is not always secure in the hands of "the best and the brightest." Furthermore, the reputation of historians for prescience has recently taken a bad battering. The pace of change in Eastern Europe caught almost everyone unprepared, and after the massacre at Tiananmen Square, one scholar confessed: "I am a chastened China watcher, as are many of my colleagues in universities and think tanks. Not since the Iranian revolution have the analysts been so surprised." He added that "no China specialist—in or out of the government—foresaw the massive setback that occurred."¹⁹

Scholars in turn have often been disappointed by their encounters with government. When Charles Frankel accepted a post at the State Department, John Kenneth Galbraith told him, "You'll find that it's the kind of organization which, though it does some big things badly, does small things badly too." There has arisen, Schlesinger has pointed out, "a certain—and understandable—skepticism on the part of intellectuals about the uses to which power seeks to put intellect. Most of the time power wants the intellectual not at all as an intellectual—that is, as a man with a critical and speculative interest in general ideas—but rather as a technician, as a man who can perform specified intellectual services."²⁰

Politicians are infinitely more likely to ask historians for confirmation of views they already hold than for examples from history that might lead them to change their opinions. Wielders of power, Otis Graham has pointed out, do not ignore history—indeed, they are historians of a sort themselves, though "quite poor ones"—but they are intent on "using the past mostly to reinforce bias and strengthen advocacy positions." As the 1966 midterm elections approached, I was asked by the Johnson White House to prepare a memo drawing comparisons to the 1942 off-year contest, and I complied with a document pointing to the vulnerability of the administration so long as it persisted in the Vietnam War. I need not tell you that, predictably, it had absolutely no effect. In 1984, I was requested to provide quotations for Walter Mondale's address in San Francisco accepting the presidential nomination. Most of those I submitted were from

¹⁸ Arthur M. Schlesinger, Jr., *The Bitter Heritage: Vietnam and American Democracy, 1941–1966* (Boston, 1966), 93; Arthur M. Schlesinger, Jr., Review of "Lessons" of the Past, *Journal of American History*, 61 (September 1974): 444.

¹⁹ David Halberstam, *The Best and the Brightest* (Greenwich, Conn., 1973); Michel Oksenberg, "Confession of a China Watcher: Why No One Predicted the Bloodbath in Beijing," *Newsweek* (June 19, 1989): 30. See, too, W. R. Connor's illuminating essay, "Why Were We Surprised?" *American Scholar*, 60 (Spring 1991): 175–84.

²⁰ Charles Frankel, *High on Foggy Bottom: An Outsider's Inside View* (New York, 1969), 11; Arthur M. Schlesinger, Jr., *The Crisis of Confidence: Ideas, Powers and Violence in America* (Boston, 1969), 83.

Franklin D. Roosevelt in the hope that Mondale would affirm liberal principles, but the only one he used came from Harry Truman: "A president . . . has to be able to say 'yes' and 'no' and more often 'no,'" because Mondale was primarily concerned with demonstrating that he was not the captive of liberal interest groups.²¹

Even when the viewpoints of scholars and officials are absolutely congruent, unanticipated consequences may ensue. In 1974, John Doar, chief counsel of the congressional inquiry into the impeachment of Richard Nixon, commissioned Vann Woodward to prepare a report on allegations of wrongdoing by American presidents throughout our history, and Woodward in turn called on me to supervise the twentieth-century section. We had only a few weeks to complete this large task, and I turned to four of my former graduate students who I knew could be counted on to do work of high quality in a hurry. In the final hours, they overran my Connecticut house and barn, but we met the deadline, as did all the others. So intent were we on seeing Nixon deposed that we sacrificed all our own projects to that end. Some have concluded that the final report was, in the words of one historian, "a major salvo in the assault on Richard Milhous Nixon," for it suggested how much more monstrous were Nixon's deeds than those of his predecessors, as indeed they were. But I have always thought that, so thoroughgoing and fairminded were the historians in revealing the many instances of wrongdoing in the past, that, if Nixon had actually stood trial, the document, in its total effect, would better have served him than the prosecution.²²

The appearance of historians in the courts, either as expert witnesses or as advisers, has also been problematic. In 1962, one of the historians who assisted the NAACP in the *Brown* case created embarrassment when, in an address at the AHA's annual convention that *U.S. News & World Report* spread over three pages, he confessed that "we were . . . sliding off facts, quietly ignoring facts, and above all, interpreting facts in a way to do what [Thurgood] Marshall said we had to do—'get by those boys down there.'" Nearly three decades later, counsel in the *Webster* case (which featured a brief signed by more than four hundred historians, of whom I was one) admitted afterward that "factors constrained our ability to 'tell the truth,'" in particular the "tension between truth-telling and advocacy." The impression that historians are objective scholars who can certify the facts of the past much like scientists reporting on the contents of a test tube proved illusory

²¹ Otis L. Graham, Jr., "The Uses and Misuses of History: Roles in Policymaking," *Public Historian*, 5 (Spring 1983): 7; personal communication, William E. Leuchtenburg to Hayes Redmon, May 1966; *Washington Post*, July 20, 1984. My views on Vietnam were stated more forcefully in a letter to Redmon on July 6, 1966.

²² C. Vann Woodward, ed., *Responses of the Presidents to Charges of Misconduct* (New York, 1974); Roper, C. Vann Woodward, 5. The four students were John W. Chambers, Robert P. Ingalls, James Boylan, and Mark I. Gelfand. The other two supervisors were Merrill D. Peterson and William S. McFeely. For the experience of one of the other historians on the project, see James M. Banner, Jr., "Historians and the Impeachment Inquiry: A Brief History and Prospectus," *Reviews in American History*, 4 (June 1976): 140–49. For the "unease" of a "resident historian" on a project with the most worthwhile aims, see John Demos, "History and the Formation of Social Policy toward Children: A Case Study," in David Rothman and Staton Wheeler, eds., *Social History and Social Policy* (New York, 1981), 301–24. Demos concluded: "Historical inquiry and policy formation made a new, awkward, and necessarily uncertain tandem. But practice may yet bring greater synchrony and increasingly substantial results"; p. 324.

when in a water rights case, one historian gave expert testimony for the plaintiff while another testified for the defense, and in the highly publicized *Sears* case, two prominent historians took opposite sides over the disposition of women to aspire to specialized job opportunities. In an important South Carolina voting-rights case, a historian who has worked intensively on Southern politics told the court that history revealed an intent by officials to discriminate racially, only to be flatly contradicted by the co-author of the country's leading textbook in Constitutional history, who denied that the historical evidence supported such a conclusion. Critics have accused scholars of cooking evidence, and one authority has even charged, with exquisitely delicate phrasing, "Expert witnesses are whores."²³

Yet, while recognizing all of these vicissitudes, I am not persuaded that historians should eschew subjects of contemporary concern or avoid the political arena, nor is that the conclusion of a number of the very scholars who have warned of the dangers of engagement. Asked how, after "three decades of controversy, criticism, and misunderstanding," he now felt about venturing to write *The Strange Career of Jim Crow*, Woodward responded: "Pressed for an answer, I would confess to feeling somewhat chastened and perhaps a bit wiser for the experience, but on the whole quite unrepentant . . . Since the historian lives in the present he has obligations to the present as well as to the past he studies." History, he said on another occasion, should not be conceived of as "a sort of verbal museum to preserve and display worthy relics of the past" or "confined to a passive role," for "the fate of ideologies, empires, and rulers hangs on historical revelations and revisions." Similarly, Schlesinger, though acknowledging that history should imbue statesmen with "a profound and humbling sense of human frailty," concluded that "we are never relieved, despite the limits of our knowl-

²³ Alfred H. Kelly, "An Inside Story: When the Supreme Court Ordered Desegregation," *U.S. News & World Report* (February 5, 1962): 88; Sylvia A. Law, "Conversations between Historians and the Constitution," *Public Historian*, 12 (Summer 1990): 14. For the water rights case, see Carl M. Becker, "Professor for the Plaintiff: Classroom to Courtroom," *Public History*, 4 (Summer 1982): 69–77; Leland R. Johnson, "Public Historian for the Defendant," *Public History*, 5 (Summer 1983): 69–77. For the South Carolina case, see McCrary and Hebert, "Keeping the Courts Honest," 115–18. McCrary was the historian who found a discriminatory motive; Herman Belz testified for the defense. In the immense literature on the *Sears* case, see *Equal Opportunity Commission v. Sears, Roebuck and Co.*, 428 F. Supp. 1264, 1308–1312 (Northern District of Illinois, 1986), affirmed 839 F. 2d 302 (1988); Joan C. Williams, "Clio Meets Portia: Objectivity in the Courtroom and the Classroom," in Theodore J. Karamanski, ed., *Ethics and Public History: An Anthology* (Malabar, Fla., 1990), 45–56; Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (Cambridge, 1988), 502–10; Thomas Haskell and Sanford Levinson, "Academic Freedom and Expert Witnessing: Historians and the *Sears* Case," *Texas Law Review*, 66 (June 1988): 1629–59; *New York Times*, June 6, 1986; Karen J. Winkler, "Two Scholars' Conflict in *Sears* Sex-Bias Case Sets Off War in Women's History," *Chronicle of Higher Education* (February 5, 1986): 1, 8; Ruth Milkman, "Women's History and the *Sears* Case," *Feminist Studies*, 12 (Summer 1986): 375–400; Alice Kessler-Harris, "Equal Opportunity Employment Commission v. Sears, Roebuck and Company: A Personal Account," *Radical History Review*, 35 (1986): 57–79; Rosalind Rosenberg, letter to editor, *Chronicle of Higher Education* (July 2, 1986): 22. Harold Green, director of the Law, Science, and Technology Program at George Washington University, is the source of the gibe about "whores" quoted in Kousser, "Are Expert Witnesses Whores?," 5. See, too, Eleanor P. Wolf, *Trial and Error: The Detroit School Desegregation Case* (Detroit, 1981); Raymond Wolters, "Advocacy, Ideology, and Objectivity: Scholars as Expert Witnesses for the Plaintiffs in School Desegregation Cases," unpublished paper. A professor of law has asserted that the efforts of Supreme Court Justices "to ground today's decisions in historians' history are fundamentally misguided." Mark Tushnet, "Should Historians Accept the Supreme Court's Invitation?" *OAH Newsletter* (November 1987): 12.

edge and the darkness of our understanding, from the necessity of meeting our obligations.”²⁴

Expert witnesses, too, have vigorously refuted the charge that they are no more than hired guns for litigants. J. Morgan Kousser, who has maintained that testifying in court and before a congressional committee on behalf of voting rights permitted him “to tell the truth and do good at the same time,” has declared, “Social scientists’ virtue is no more at stake as they walk down the dark alleys of policy relevance than it is on the brightly-lit streets of the campus.” Similarly, Peyton McCrary has affirmed that “the courtroom helps keep the academics honest,” for “if experts do not testify fully, logically, convincingly, and honestly, then the process of cross-examination by skillful attorneys is likely to expose their faults.” “The standards of the courtroom,” he deduced from his own experience, “are as high as those of academe.” Despite, or more likely because of, the bitter experience of the *Sears* case, a Conference on Women’s History and Public Policy in 1989 explored “mechanisms for enhancing communication between historians of women and those in the political arena and in the courts.”²⁵

Granted that their capacities are not unbounded, history professors do not have to remain immured behind campus walls. They can reach out to their colleagues not only in national but in state and local governments, as well as in the private sector; they can collaborate with teachers in elementary and secondary schools doing the indispensable work of instructing the young in understanding the past; and they can, in the tradition of Macaulay and Parkman, write not just for one another but for a literate public.

Such an obvious agenda, though, does not begin to encompass what is expected of us. As John Hope Franklin has pointed out:

Let a person move into a group of people and be introduced as an historian; and someone will raise a question that he knows is at least as profound as any that Socrates ever raised. To the historian it will sound like “Please, Sir, say something historical!” The actual words, carefully articulated, will be, “Please, Sir, tell me what the next four years will provide in the way of history.” It is no use to reply, “I am not a soothsayer; I am an historian.” For the reply is likely to be, “That is precisely why I put the question to you and not to someone else.”²⁶

²⁴ C. Vann Woodward, *The Strange Career of Jim Crow* (New York, 1974); Woodward, *Thinking Back*, 98; Woodward, *Future of the Past*, xi; Arthur Schlesinger, Jr., “On the Inscrutability of History,” *Encounter*, 27 (November 1966): 17.

²⁵ Kousser, “Are Expert Witnesses Whores?” 7, 19; Peyton McCrary, “Keeping the Courts Honest,” 128; Alice Kessler-Harris and Amy Swerdlow, “Report on the First Conference on Women’s History and Public Policy,” *Perspectives*, 28 (May–June 1990): 10; Final Report to the Ford Foundation on the Conference on Women’s History and Public Policy, June 15–17, 1989, Sarah Lawrence College, Bronxville, N.Y. I am indebted to Professor Swerdlow for a copy of this report. Despite misgivings, the historians involved in the *Webster* case reaffirmed their belief that they had taken the right course. There is an excellent discussion in *Public Historian*, 12 (Summer 1990). See, too, Allan M. Brandt, “Writing Policy-Directed History,” unpublished paper presented at the History of Science Society meetings, Madison, Wisconsin, November 3, 1991.

²⁶ Franklin, “The Historian and the Public Policy,” 309. One scholar has concluded, “In America, at least, although the pursuit of learning may well be its own reward, it cannot for long among the otherwise occupied citizens be its own justification.” Robert A. McCaughey, *International Studies and Academic Enterprise: A Chapter in the Enclosure of American Learning* (New York, 1984), 255. For the observation that “in the nineteenth century more than the twentieth, history was written for and read

Making an effort to meet such expectations, if not as prophets then as guides to comprehending the sources of the predicaments of our time, may conceivably be of value not just to the nation but to scholars as well. "The vast majority of academics in traditional arts and science disciplines rarely venture forth to confront, enlighten, or change the world," observed a historian who has taken a different course. "Monasticism does have its shortcomings. It contributes to excessively narrow specialization; it impedes teaching by making it too removed from the world most students hope to occupy; it denies the practical world the benefit of academic knowledge and thought; and it denies professors the benefit of having their work tested in the world of practice."²⁷

In a presidential address to this association thirty-two years ago, Allan Nevins, taking note of the question whether "the political historian who has never testified before a congressional committee, or written a speech for a governor or mayor, or haunted the city hall for a year, is not handicapped as compared with the man who has," recalled Macaulay's remarks on Gibbon, who had been a militia officer and a member of Parliament:

We have not the smallest doubt that his campaigns, though he never saw an enemy, and his parliamentary attendance, though he never made a speech, were of far more use to him than years of study and retirement would have been. If the time he spent on parade and at mess in Hampshire, or on the treasury bench and at Brooks's, during the storms which overthrew Lord North and Lord Shelburne, had been passed in the Bodleian library, he might have avoided some inaccuracies; he might have enriched his notes with a greater number of references; but he could never have produced so lively a picture of the court, the camp, and the senate house.²⁸

Quite apart from such considerations, scholars need to embrace an active role in national affairs because they have a vital professional stake in doing so. I would hate to think of what might have happened over the past several years if we had not joined in the National Coordinating Committee for the Promotion of History and had not had the benefit of the talents of Page Putnam Miller. At this last session of Congress alone, under her leadership, the committee, an umbrella organization of fifty-one groups, was able to achieve legislation requiring the State Department to set up a systematic program to declassify documents, and only this morning we met at breakfast to discuss legislative strategy on bills before Congress of paramount significance for scholars, including measures reauthorizing the National Historical Publications and Records Commission's grants and clarifying "fair use" of unpublished copyrighted material by modifying a recent court

by large numbers of people other than academics," see Mary O. Furner, *Advocacy and Objectivity: A Crisis in the Professionalization of American Social Science, 1865-1905* (Lexington, Ky., 1975), 297.

²⁷ Carl Brauer, "More Scholars Should Venture Forth to Confront, Enlighten, or Change the World," *Chronicle of Higher Education* (March 14, 1990): B2. The former chairman of a state committee for the humanities urged greater collaboration between the academy and the public, not least because "it is through such engagements that scholars can discover whether the insights gained in their research are really communicable, pertinent, and lively." Ron Perrin, "Humanists Must Forge Links between Their Work and the Public," *Chronicle of Higher Education* (September 27, 1989): A56.

²⁸ Allan Nevins, "Not Capulets, Not Montagus," *AHR*, 65 (January 1960): 264-65.

decision that, as Anne Firor Scott has said, is “a time bomb waiting to blow up all our work in primary sources.”²⁹

Charles Frankel once put this matter in a larger frame. “The right not simply to dissent but, if one pleases to be indifferent; the right to be private; the right to be useless from every respectable point of view; the right to be irreverent about what is officially sanctified—when have these rights ever been safe from the crowd?” he asked. “When have they been safe even from other intellectuals?” Frankel continued:

It may once have been possible for scholars to guard their independence by keeping their distance from power. It may be possible for individual scholars to do that still. But it is not possible for the scholarly community as such to maintain its independence by running away from government. For key decisions that affect scholarly independence will be made in any event. And if they are made without the participation of men and women who know something about the nature and necessary conditions of scholarly and intellectual life, they cannot be expected to be the right decisions.³⁰

My conviction that historians have something to contribute to decision making rests primarily, though, not on such self-interested grounds but on a much more fundamental proposition: that movers and shakers act in part because of the history they carry around in their heads. In 1947, Secretary of State George Marshall declared, “I doubt seriously whether a man can think with full wisdom . . . regarding certain of the basic international issues today who has not at least reviewed in his mind the period of the Peloponnesian War and the Fall of Athens.” The president under whom Marshall served had a cruder notion of the past. “The oligarchy in Russia,” Harry Truman wrote his daughter, “is no different from the Czars, Louis XIV, Napoleon, Charles I and Cromwell. It is a Frankenstein dictatorship worse than any of the others, Hitler included.” Still more notorious was Lyndon Johnson’s preoccupation with historical analogy. The lesson Johnson applied in Southeast Asia came from what he had absorbed from the 1938 Munich crisis—that if leaders shirked their responsibilities abroad, they only postponed their problems, which wound up being worse. “We’re not,” he vowed, “going to have any men with any umbrellas.”³¹

²⁹ Circular letter, Anne Firor Scott to “Dear Colleague,” November 1991, in the possession of the author.

³⁰ Charles Frankel, “The Political Responsibility of the Intellectual,” in Paul Kurtz, ed., *Moral Problems in Contemporary Society: Essays in Humanistic Ethics* (Buffalo, N.Y., 1973), 174–75. For Frankel’s views, see William E. Leuchtenburg, “Charles Frankel: The Humanist as Citizen,” in John Agresto and Peter Riesenber, eds., *The Humanist as Citizen* (Research Triangle Park, N.C., 1981), 228–54.

³¹ W. Robert Connor, *Thucydides* (Princeton, N.J., 1984), 3; Harry S. Truman to Margaret Truman, March 3, 1948, quoted in Margaret Truman, *Harry S. Truman* (New York, 1973), 360. Hugh Sidey, *A Very Personal Presidency: Lyndon Johnson in the White House* (New York, 1968), 218. Senator J. William Fulbright replied to Johnson’s insistence that the situation in Vietnam corresponded to that at Munich by saying, “The treatment of slight and superficial resemblances as if they were full-blooded analogies—as instances of history ‘repeating itself’—is a substitute for thinking and a misuse of history”; Alfred Steinberg, *Sam Johnson’s Boy: A Close-Up of the President from Texas* (New York, 1968), 788. Arthur Schlesinger, Jr., has written: “I trust that a graduate student some day will write a doctoral essay on the influence of the Munich analogy on the subsequent history of the twentieth century. Perhaps in the end he will conclude that the multitude of errors committed in the name of ‘Munich’ may exceed the original error of 1938”; Schlesinger, *Bitter Heritage*, 89. See, too, Chalmers Roberts Oral History, Lyndon B. Johnson Library, Austin, Texas. For the Supreme Court’s misuse

It does not seem too much to suppose that historians, sensitive to the nuances of metaphor, can better that record. We ought to take on such assignments with full recognition that history is not an exact science and that historians are not seers. Still, as Carl Degler has asserted, "A recourse to history may well save governments and other agencies from ill-considered acts of policy, even if a knowledge of the past cannot tell us what action to take." Although historians have shown themselves to be fallible, and can be counted on to be so again, Joseph Strayer has said of history: "We may go wrong in following the clues which it offers, but we would be lost without them . . . History at its best gives us a real chance of reacting sensibly to a new situation. It does not guarantee the correctness of our response, but it should improve the quality of our judgment." Strayer reasoned: "A rough parallel may be found in certain card games. There is almost no chance that one distribution of cards will be repeated in a subsequent deal in bridge. Yet a man who has played several thousand hands of bridge should be able to make intelligent decisions and predictions even though every deal presents a new situation." Furthermore, as Alan Brinkley has said, "Illuminating the past is a way of protecting individuals and society from the glib and self-serving analogies that politicians routinely use to justify their own interests," and "a knowledge of history arms one to consider critically the claims of political figures."³²

The issue, I believe, is not whether historians should intervene but whether they can do so more effectively. Ernest May and Richard Neustadt have offered a manual on how policymakers can avoid the trap of beguiling historical analogies that has opened up a debate on that vexing matter, and at the Georgia Institute of Technology Robert McMath has introduced a course, taught at the Carter Library, on "The Uses of History for Policy-Makers" that could be a model for others. More than a decade ago, twenty-one prominent historians led by Robert Kelley urged President Carter to institutionalize a historical presence in the federal government, perhaps on the model of the Council of Economic Advisers. It will be objected that history is not as technically refined a subject as economics and that historians diverge widely in their views, but it has also been said that if all the economists in the world were laid end to end, they still would reach no conclusion; yet the CEA has proven to be a constructive innovation. The notion has some pitfalls—notably the possibility that historians in such an agency might become captives of the reigning administration—and it might well prove preferable to adopt the proposal in a more modest form. But it deserves more scrutiny than it has received. Governments might well benefit from an enhanced role for historians not because they are good predictors but because they are men and

of history, see Alfred H. Kelly, "Clio and the Court: An Illicit Love Affair," *Supreme Court Review* (1965): 119–58.

³² Carl N. Degler, "Is the New Social History Threatening Clio?" *OAH Newsletter*, 16 (August 1988): 5; Joseph R. Strayer, "Introduction," in Strayer, ed., *The Interpretation of History* (Princeton, N.J., 1943): 14–15; Alan Brinkley to the author, January 13, 1992. "Historical generalizations," Schlesinger has written, "will enlarge the wisdom of the statesman, giving his responses to the crises of the moment perspective, depth and an instinct for the direction and flow of events"; *Bitter Heritage*, 83.

women skilled in retaining institutional memories, perceiving the complexity of problems, and placing events in the stream of time.³³

In sum, in considering the long warfare between historians who favor engagement and those who oppose it, I would join issue on the one point on which they agree—that their positions are irreconcilable. Instead, I see a creative tension between the two attitudes. Scholars would do well to give a respectful hearing to both groups, for neither holds a monopoly of the truth. One can agree that history has value wholly apart from any utilitarian end it serves without accepting the conclusion that historians must refrain from public involvement, and one can acknowledge that historians have an obligation to their community without dismissing the sage admonitions that the skeptics raise.

The historians who reject involvement might well ask themselves if they truly believe that, devoting their lives as they do to the study of history, they have nothing to contribute to the compelling public concerns of their only time on earth. For all his criticism of the progressive school, Richard Hofstadter generously conceded that “at their best, the interpretative historians have gone to the past with some passionate concern for the future,” and even Julien Benda endorsed certain public actions by intellectuals: “When Gerson entered the pulpit of Notre Dame to denounce the murderers of Louis d’Orléans; when Spinoza, at the peril of his life, went and wrote the words ‘Ultimi barbarorum’ on the gate of those who had murdered the de Witts; when Voltaire fought for the Calas family; when Zola and Duclaux came forward to take part in a celebrated lawsuit (the Dreyfus affair).”³⁴

On the other hand, those of us who do take part in public affairs need constantly to remind ourselves that we are not omniscient, and that we must never, no matter how worthy the cause, compromise our commitment to, in John Higham’s words, “the simple axiom that history is basically an effort to tell the truth about the past.” We who are professors ought to remember that there are advantages, not only for ourselves but for society, to the detachment the campus affords us, and that unceasing involvement may diminish our capacity to see the world more clearly. When we do speak out, and we should choose those times

³³ Richard E. Neustadt and Ernest R. May, *Thinking in Time: The Uses of History for Decision-Makers* (New York, 1986); Gordon Wright *et al.* to Jimmy Carter, November 3, 1976. See, too, *OAH Newsletter* (January 1977): 4. At Kelley’s behest, the Executive Board of the Organization of American Historians sent a similar communication to President Ronald Reagan. Executive Board Minutes, April 1, 1981; Richard Kirkendall, “Executive Secretary’s Report,” *OAH Newsletter* (July 1981): 11. I am indebted to Kirkendall, Arnita Jones, and Sharon Caughill for locating these materials for me. For a similar recommendation, see Ernest R. May, “Lessons” of the Past: *The Use and Misuse of History in American Foreign Policy* (New York, 1973), 172. One critic has objected, “A History Office in the West Wing . . . would be ‘on the team,’ with the inevitable narrowing of vision and independence”; Graham, “Uses and Misuses of History,” 13. Graham, though, has shown the potentialities as well as the limitations of the role of historians in policymaking in “Intellectual Standards in the Humanities,” in Daniel Callahan, Arthur L. Caplan, and Bruce Jennings, eds., *Applying the Humanities* (New York, 1985), 261–69, and he has provided an example of sophisticated analysis of a policy question by a historian in *Losing Time: The Industrial Policy Debate* (Cambridge, Mass., 1992). I have also found informative Page Putnam Miller, “History in Government and Public Policy,” unpublished paper. For the difficulties but also the opportunities historians encounter when they try to predict, see Louis Gottschalk, *Understanding History: A Primer of Historical Method* (New York, 1951), 264–71.

³⁴ Hofstadter, *Progressive Historians*, 465; Benda, *Treason of the Intellectuals*, 50. For the responsibility of the scholarly community, see George McT. Kahin, “A Polarization of Knowledge: Specialization on Contemporary Asia in the United States,” *Journal of Asian Studies*, 33 (August 1974): 515–22.

wisely, we must take care to distinguish between doing so as historians and doing so simply as politically active citizens. Above all, we should take care not to create an atmosphere in the classroom in which views that diverge from our own cannot freely be voiced, and we should respect the rights of others in the profession to express beliefs contrary to our own or to remain silent.³⁵

The intellectual, Charles Frankel wrote, “may and should take sides in the political struggles of his time, but there is likely to be an edge of irony or regret in his attitude when he does so.” It seems inevitable that historians will always feel this tension—“caught,” in Hofstadter’s words, “between their desire to count in the world and their desire to understand it.” Their “passion for understanding” moves them toward “detachment” and “neutrality,” but “the terrible urgency of our political problems . . . plays upon . . . their desire to get out of history some lessons that will be of use in the world.” For my own part, I would commend the message Emerson left us in his celebrated Phi Beta Kappa oration, “The American Scholar”—that “action is with the scholar subordinate, but it is essential,” and that “there can be no scholar without the heroic mind.”³⁶

³⁵ Higham, *History*, 132.

³⁶ Charles Frankel, “Definition of the True Egghead,” *New York Times Magazine* (October 21, 1956): 62; Hofstadter, *Progressive Historians*, 464; *The Collected Works of Ralph Waldo Emerson* (Cambridge, Mass., 1971), 1: 59. C. Vann Woodward’s biographer has written that “Woodward’s life would be taken up with a continuing effort to find a proper balance between the competing masters of political causes and of disinterested scholarship. All scholars who seek societal reform ultimately face the dilemma posed by the contradictions between the two impulses . . . Scholarship requires a degree of detachment from the concerns of the moment, while genuine activism permits little time for reflection on events and characters of the past ‘irrelevant’ to contemporary causes”; Roper, *C. Vann Woodward*, 59–60. For the travail of one historian who sought to combine scholarship with involvement in public affairs, see Paul M. Evans, *John Fairbank and the American Understanding of Modern China* (New York, 1988).

Social Insurance and Public Assistance: The Influence of Gender in Welfare Thought in the United States, 1890–1935

LINDA GORDON

The . . . method of handling as one problem all cases involving needy mothers, regardless of the cause of their difficulties, is as ill considered as would be the treating of all sick persons alike.

Barbara Nachtrieb Armstrong, *Insuring the Essentials* (1932).

All insurance is a substitution of social, co-operative provision for individual provision . . . known as the theory of distribution of losses and the subsequent elimination of risk.

I. M. Rubinow, *Social Insurance* (1913).

IN THIS ESSAY, I SUGGEST THAT WELFARE REFORMERS in the United States had recognizably gendered views of what a welfare state should offer, and thus of what a good society, in the industrial period, should be like, and that these gendered visions help explain why we have the welfare system we do. In identifying the influence of gender, I do not argue that these visions were dichotomized between men and women. There were commonalities, based on other social experiences, at least as important as the differences. My purpose is not so much to distinguish male from female as it is to illustrate the importance of asking questions about gender, questions that illuminate similarity as well as difference.

We can see these visions by examining the welfare thinking that produced the U.S. Social Security Act of 1935. Often considered the foundation of the American welfare state, Social Security was an omnibus law that established several programs, prominent among them Old Age Insurance, Unemployment Compensation, and Aid to Dependent Children. It also included aid to the elderly, the blind, and certain other disabled groups. Welfare scholars usually divide these programs into two categories: the first two programs, comprising “social insurance,” are more generous and popular; the remainder, called public

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assistance, are stingier and stigmatizing. In the last two decades, scholars have also recognized that these programs divide along the lines of sex and race. The better programs, Old Age Insurance (now OASI) and Unemployment Insurance (UI), disproportionately serve white men, and the inferior assistance programs, women and minority men; this stratification was even greater when the law was designed. The first-track programs have become so respectable that they are not even considered by most Americans as “welfare”—this word now being a pejorative. Their stipends are relatively high, they are offered without means-testing, and they are received as a matter of entitlement without the requirement that the recipient submit to personal supervision. The second-track programs continue the old poor-law tradition: the stipends are low, the standard awards place recipients below the poverty line and are not designed to allow them to attain a respectable working-class standard of living.¹ The largest and best-known example of such a program is Aid to Dependent Children (now AFDC). Its awardees are stringently means-tested, thus encouraging—one might say requiring—a recipient to cheat if she desires to better herself and her children; it has required the recipient to submit to personal supervision of her private life; and it carries a deep stigma.²

Scholars have identified this stratification in the structure of our welfare state but not in the thinking that gave rise to it. The social insurance programs developed from an almost exclusively male current of thought, while public assistance developed from an integrated perspective, with the advice of many female leaders. The OASI and UI programs were designed by men, the remainder primarily by women. This division of labor was achieved by mutual assumption, without a struggle, and with mutual support; indeed, the division of labor was such a given to those involved that they did not explore their differences but tended to see mainly their areas of agreement. They were right to do so, because they shared a fundamental premise: a welfare state should protect the

¹ The “poverty line” is itself a highly malleable construction dependent on decisions by elites about how many should be allowed to appear poor; it is not an “objective” indicator of well-being. See Diana Karter Appelbaum, “The Level of the Poverty Line: A Historical Survey,” *Social Service Review*, 51 (September 1977): 514–23.

² Barbara J. Nelson, “The Origins of the Two-Channel Welfare State: Workmen’s Compensation and Mothers’ Aid,” and Diana Pearce, “Welfare Is Not for Women: Why the War on Poverty Cannot Conquer the Feminization of Poverty,” both in *Women, the State, and Welfare*, Linda Gordon, ed. (Madison, Wis., 1990); Linda Gordon, “What Does Welfare Regulate?” *Social Research*, 55 (Winter 1988): 609–30. This dichotomy can be and has been described in other terms as well, and this gender interpretation is meant to supplement, not to replace, other characterizations. For example, the second-track programs grew out of voluntary charity and “friendly visiting,” with roots in the English poor law and in the American tradition of charity organization. The first-track program designs were often modeled on German or other European statist systems of providing for and disciplining the proletariat. The second track grew from private charity, the first was primarily state-operated from the start. For early discussion of this dichotomy, see Eveline M. Burns, *The American Social Security System* (New York, 1949), 28–43; Jacobus tenBroek and Richard B. Wilson, “Public Assistance and Social Insurance—A Normative Evaluation,” *UCLA Law Review*, 1 (April 1954): 237–302; Richard M. Titmuss, “The Role of Redistribution in Social Policy,” *Social Security Bulletin*, 28 (June 1965): 14–20; Joel F. Handler and Margaret K. Rosenheim, “Privacy in Welfare: Public Assistance and Juvenile Justice,” *Law and Contemporary Problems*, 31 (1966): 377–412. For a more recent discussion, see Jill S. Quadagno, “From Poor Laws to Pensions: The Evolution of Economic Support for the Aged in England and America,” *Milbank Memorial Fund Quarterly/Health and Society*, 62 (1984): 417–46; Daniel Patrick Moynihan, “Our Poorest Citizens—Children,” in *Focus*, newsletter of the University of Wisconsin, Madison, Institute for Research on Poverty, 11 (Spring 1988): 5.

family-wage system. Still, welfare reformers did differ about the meanings of that system and about how to fix its breakdowns. By scrutinizing the thinking of welfare proponents between 1890 and 1955, with particular attention to its gendered content, we gain new understandings of how our welfare, gender, and family systems have changed in the course of the past century.

Recognizing this division of labor in welfare thought substantially modifies one influential feminist critique of the welfare state: that AFDC is inferior because it was designed for women by a "patriarchal" state.³ Certainly, men were always the powers in government, but, in fact, a network of female reformers had the defining influence in the design of Aid to Dependent Children. Why did women design inferior programs for women? Were they dupes, did they identify with men, were they convinced that women deserved less? Hardly. The women who designed our welfare programs were the inheritors of the women's rights legacy, and they were aware of sexual inequities. The following discussion may help to answer these questions, although its purpose is less to answer questions than to illuminate neglected aspects of how our current welfare system emerged.

This essay is situated in two fields of theoretical and empirical scholarship: welfare history and gender studies. Ignoring the gendered assumptions of much welfare legislation, some histories of the welfare state have failed to examine what welfare designers took for granted. For example, by taking the normative family as a given, welfare advocates construed poverty mainly as a result of lapses in male breadwinning and made men the designated objects of help.⁴ Both welfare designers and scholars missed not only historical changes in family economies but also evidence that the norms seldom if ever matched the reality. "Gender," meanwhile, has often been used as a synonym for "female," maintaining the invisibility of maleness. The resulting scholarship has sometimes somersaulted into a kind of female Whiggism: crediting the value of such welfare programs as we have to women's nurturant inclinations, while the limitations of our welfare state are derived from male individualism. This approach is limited by both a romantic view of women's generosity and an overly dichotomized view of gender, which in turn assumes a kind of unity among women that was never present.⁵ Specifically, this supposed unity denies that women's agency also derives from other aspects of their social position, such as class, religion, and race. In arguing for a more complicated interpretation, I do not retreat from claims that gender

³ For example, see Sylvia A. Law, "Women, Work, Welfare, and the Preservation of Patriarchy," *University of Pennsylvania Law Review*, 131 (May 1983): 1251–1331; Carol Brown, "Mothers, Fathers, and Children: From Private to Public Patriarchy," in *Women and Revolution: A Discussion of the Unhappy Marriage of Marxism and Feminism*, Lydia Sargent, ed. (Boston, 1981), 239–68; Mimi Abramovitz, *Regulating the Lives of Women: Social Welfare Policy from Colonial Times to the Present* (Boston, 1988). One of the errors of this view, which I do not have space to discuss here, is that second-track programs also include general relief, in which men are the main recipients. I am indebted to Joel Handler for this important point.

⁴ One of the most recent comparative studies of the development of welfare states lists four sources of need: disability, old age, disease, and unemployment; Abram de Swaan, *In Care of the State: Health Care, Education and Welfare in Europe and the USA in the Modern Era* (Cambridge, 1988), 177. It is surprising that a recent book would not have added to this list such conditions as divorce and out-of-wedlock parenthood. As long ago as 1900, 9 to 10 percent of children in the United States lived with single parents, almost always women. See Linda Gordon and Sara McLanahan, "Single Parenthood in 1900," *Journal of Family History*, 16 (1991): 97–116.

⁵ Linda Gordon, "On Difference," *Genders*, 10 (Spring 1991): 91–111.

must be a primary unit of analysis but argue against conceiving gender as a fixed, dichotomous, and oppositional dualism.

The comparison of male and female visions does not imply that there is a universal gender system or that gender was necessarily the determining or even dominant factor in constructing these visions. The subject here is the influence of gender among particular groups in a limited time period. Moreover, the groups were by no means homogeneous. There were substantial disagreements among them and considerable changes in their views over time. This analysis screens off such differences for the sake of a comparison. Still, by treating as one a disparate network extending over forty-five years, we gain a bird's-eye perspective, making manifest certain broad patterns missed in monographic studies.

THE WELFARE ADVOCATES DISCUSSED HERE do not constitute a representative sample, and I did not set out to define a group of individuals for study. But they accumulated so noticeably in my notes, and they so frequently wrote letters and memos to each other, that they seemed to demand consideration as a network. Only after I began seeing the patterns in their thought did I systematize information about them, counting individuals and collecting biographical data. The end result was seventy-six white women and seventy-six white men who were national leaders of welfare reform movements from 1890 to 1935.⁶ (They are listed in the Appendix, along with a fuller explanation of how they were chosen.) These include the leaders of national organizations in the field such as the American Association for Labor Legislation and the National Consumers' League, and other individuals who were prominent for their advocacy of public social and economic provision. I excluded persons whose primary contributions to the cause were made as elected officials or as philanthropists. I make no claims to have assembled an inclusive or representative list, but, on the principle of saturation, I doubt that the addition of more people would alter my findings.

The segregation of these individuals by sex and race was largely their own doing. Even those married to others on my list carried on a correspondence primarily with persons of their own sex. Most of these prominent whites excluded blacks from their networks and counsel. In this forty-five-year period, African Americans were still mainly concentrated in the South, overwhelmingly disfranchised, with little influence on government at any level. As a result, their campaigns for improving the welfare of blacks took different forms—more often

⁶ It was completely accidental that I ended up with an equal number of men and women. Collecting a group of welfare advocates to study was a strategy suggested by Allen F. Davis, *Spearheads for Reform: The Social Settlements and the Progressive Movement 1890–1914* (New York, 1967), 33–39; Susan Ware, *Beyond Suffrage: Women in the New Deal* (Cambridge, Mass., 1981), appendix; and Judith Ann Trolander, *Professionalism and Social Change: From the Settlement House Movement to Neighborhood Centers, 1886 to the Present* (New York, 1987). Stephen Kalberg appears to have done something similar in "The Commitment to Career Reform: The Settlement Movement Leaders," *Social Service Review*, 49 (December 1975): 608–28, but he so completely ignores gender as to not even mention whether he included women, men, or both.

building private institutions, because their campaigns for public provision were much less successful. (I have discussed black welfare activism elsewhere.⁷)

Having gathered biographical information about these welfare leaders, I found some significant patterns. They were, as one might expect, from elite backgrounds on the whole. This is indicated by their high levels of education, the proportion with wealthy or professional parents, and their typically Protestant, North European backgrounds. Most had allegiances to the Republican Party and the temperance movement. Most took their Christianity very seriously and considered their reform work part of a Christian moral vision. Some evidence suggests that the women were, on average, from more elite backgrounds than the men—no doubt in part because a lack of access to most vocations sent a large proportion of all educated women into reform activities. The men were slightly more highly educated than the women (of men, 96 percent had a college education and 84 percent had been to graduate school; among women, the figures are 86 and 66 percent), but the women were more exceptional in their educational achievements than the men because fewer women in society at large had higher education.⁸ Two-thirds of the women had elite parents, one-third of the men.⁹ The women's network also included fewer immigrants or non-Protestants than the men's.¹⁰ One-fifth of the men were Jewish, and in the later part of the period, including men born after 1880, the proportion goes up to one-third. Half the Jewish men were immigrants, and many of these and the native-born Jews were from poor backgrounds, often with radical political inclinations. By contrast, the nine Jewish women were primarily from wealthy German backgrounds and were born in the United States. A third of the unmarried women were not usually employed—that is, they were able to live without earning, another indication of their privileged backgrounds. It is safe to assume that this figure underestimates those who could have lived on family money, since many of these women chose to take jobs even without economic necessity.¹¹

Occupational comparisons between the men and the women are necessarily

⁷ Linda Gordon, "Black and White Visions of Welfare: Women's Welfare Activism, 1890–1945," *Journal of American History*, 78 (September 1991): 559–90; this study is also available as a Discussion Paper from the Institute for Research on Poverty, University of Wisconsin, Madison.

⁸ Ratios of male to females earning college degrees throughout this period are as follows, computed from the Bureau of the Census, *Historical Statistics of the United States* (Washington, D.C., 1975), Part 1, 386.

<i>Year</i>	<i>Ratio</i>
1890	100 : 23
1900	100 : 23
1910	100 : 28
1920	100 : 51
1930	100 : 66
1940	100 : 70

⁹ I considered as elite those of substantial wealth and/or high professional position; exact figures were 69 percent of women and 34 percent of men.

¹⁰ Thirteen percent of the men were foreign-born, only 8 percent of the women.

¹¹ Of the ten women who were divorced, widowed, or separated, only four were employed.

limited in meaning, since virtually all men of this class grew up assuming that they would find a career, and virtually all these men had found a way to integrate welfare advocacy and salaried jobs. The women often worked as volunteers, although the proportion of women employed—61 percent—was far beyond the national average, which moved from 18 to 25 percent in this period. Allowing for these gender differences, a significant occupational difference remained. Most of the men were academics, and this proportion grew to 70 percent among the younger men (those born after 1880).¹² While the majority of the men had participated in social work, most had done so temporarily, before committing themselves to their other professions. By contrast, virtually all the women had participated in some social work activity, and two-thirds were social workers all their lives; only one-fifth were academics.¹³ There is nothing surprising about this, given the widespread discrimination against women in academia and the development of social work as a female and low-status profession, but important here are the different influences on men and women working for welfare. The women were much more likely to have had long-term direct contact with the poor, through settlement houses or relief agencies. All except the oldest women personally experienced the transformation of social work from a volunteer to a professional activity, beginning their careers as volunteers for charity or reform groups, ending as salaried workers for the same or similar groups. Women's careers had arisen directly from their charitable and reform activism, and their identities were strongly influenced by their goals and their self-definition as altruists. Men's identities were far more defined by their professions, and men with an equivalent passion for social justice and mercy fulfilled that mission through a profession, experiencing from their youth the pressure for discipline, specialization, and achievement.¹⁴ Paradoxically, more men in this network were actively religious than women. This suggests yet another difference in what drew men and women to reform activism: for women, the activism *was* the career¹⁵; men's welfare activism sometimes represented sacrifice of a more conventional road to success, a choice that may have been connected to their religiosity. The unconventional reform careers they chose may also have served as routes to higher status for the upwardly mobile immigrant Jews among them.

Because proper family life was important to the welfare vision, I thought it useful to investigate the domestic circumstances of welfare reformers, and here, too, there was a significant gender difference. Virtually all (91 percent) of the men were married, and two-thirds (68 percent) had children. Only 34 percent of the

¹² The men born before 1880 were most commonly lawyers, ministers, or economist-statisticians; the younger men were most often administrators or academics. In other words, they made a transition characteristic of all educated men from being self-employed to salaried workers.

¹³ I am here defining social work the way they did, to include both salaried and volunteer activity.

¹⁴ Robyn Muncy, *Creating a Female Dominion in American Reform, 1890–1935* (New York, 1991), 70–71. Muncy argues that Edith Abbott's main motivation was careerist and that she was forced into reform by the lack of professional opportunity for women. While this is true, it is also true that Abbott united her career and social goals. Even had her primary motivation been careerist, she was disciplined into a reform orientation by her social milieu, the women's political culture in which she lived.

¹⁵ Ellen Fitzpatrick, *Endless Crusade: Women Social Scientists and Progressive Reform* (New York, 1990), 176, remarks on this in relation to the small group she studied.

women had ever been married, only 28 percent had children, and only 18 percent remained married during their peak political activity. These figures verify patterns well known to social historians of the period. The years in which these women were young (approximately 1870 to 1915) represented an important transitional era for privileged women, who were entering the public sphere through professions and activism in larger numbers than ever before; but it remained socially very difficult to combine this public-sphere activity with marriage. The difficulty arose not from the stresses of the “double day” that working-class women faced, because these women, mainly middle-class or above, could afford to hire help with household and child-care duties. Instead, the barrier to combining marriage and “career” was cultural. Men and most women of this class expected wives to remain domestic. The letters, diaries, and fiction of this class and generation of women indicated that, as they approached adulthood, they faced a choice between two mutually exclusive options. (It should be noted that this sense of choice was a distinctly white as well as middle-class experience. Black women of comparably elite status in respect to their communities felt it respectable to combine the two and mainly did so.¹⁶)

Thus in the organizations and committees on which they sometimes worked together, married men encountered single women. These women were middle-aged before they reached positions of importance in their reform area, in contrast to the men, who were often younger. The women were often perceived as spinsters. It is true that they were not involved in the heterosexual marriage-and-family experience of most women, but many were in some kind of emotional or sexual partnership with other women. Since this alternative pattern of coupling was not widely recognized at the time, this group did not appear to represent either an alternative to or a rejection of conventional family life.¹⁷ Rather, they were considered exceptions and, by the 1930s, increasingly as unfortunate exceptions, women who had been unable to achieve, or who had sacrificed, the joys of family.

As a group, these welfare advocates were simultaneously close and riven with disagreements, both personal and political.¹⁸ Most of them saw others in their network frequently. The segregation by sex was mutual, but its meanings were somewhat different for men than for women. The welfare “old boy network” reproduced existing professional and governmental patterns in which men occupied almost all positions of authority. Some of these men turned to women such as Grace and Edith Abbott, Molly Dewson, Florence Kelley, Mary Van Kleeck, and Lillian Wald for information or effective action; it is evident that men

¹⁶ By contrast with these white female welfare activists, 85 percent of black women I studied were married, only 14 percent widowed or separated; Gordon, “Black and White Visions of Welfare,” 568, 574–75.

¹⁷ Twenty-eight percent were in relationships with other women that might have been called “Boston marriages,” long-term coupled relationships. My figure is conservative since I counted only those women for whom I could identify a partner. Today, some of these women might be called lesbian, and there is a spirited discussion among historians as to whether it is appropriate to apply this more recent word to this generation and this kind of relationship, but that issue is not relevant to my argument.

¹⁸ For example, the men divided over alternate models for social insurance funding in the 1920s and 1930s, the women over their attitudes toward an Equal Rights Amendment in the same period.

such as John Andrews, Louis Brandeis, John Commons, Paul Raushenbush, and Carroll Wright understood the extraordinary abilities of many of the women in this group. But the men, on the whole, did not promote women to positions of public leadership. The “girls’” network, for this is what the women often called themselves, was probably stronger and more personally intimate. With many members not tied to conventional families, they spent much of their leisure time together—weekending and vacationing at summer homes, nursing others through illness, living with each other privately, in settlements, or in their clubs.

The strong sense of community among the women and their largely nonprofessional, volunteer status kept alive a somewhat autonomous women’s political culture even after they entered mainstream politics. All but the youngest women were advocates of women’s rights, and at least half had worked for female suffrage. For most, however, suffrage was not their primary mission by the early twentieth century; instead, they were “social feminists,” what Europeans might have called social-democratic feminists, motivated by their concern for the women and children of less fortunate classes. Still, their reform strategy had grown out of the experience of disfranchisement and exclusion from the high professions, and most of them—until 1932—avoided party politics in favor of nonpartisan civic organization. Their practice was characterized by close communication and joint planning between those in public office and those in civic organizations, to a higher degree than the men’s. White middle-class women’s pattern of nonemployment conditioned this strategy, for these women supplied much of the volunteer activism on which welfare leaders depended. To cite just one example, 3,000 volunteer club women did the interviewing in the birth registration surveys by the U.S. Children’s Bureau.¹⁹

At the end of the nineteenth century, women like these were the acknowledged force behind charity work, and some had even made their way into its national leadership. As charity became “social work” during the Progressive Era, it remained one of the quintessential female professions. Women such as Josephine Shaw Lowell, Mary Richmond, Sophonisba Breckinridge, and Edith and Grace Abbott powerfully influenced, possibly dominated, the field, from the 1880s through the Great Depression. In 1901, University of Chicago sociology professor Charles Henderson’s textbook on social work and social reform assumed a female readership.²⁰ Men as well as women conceived of the welfare state as female. Alexander Johnson, in his presidential address to the National Conference of Charities and Correction, referred to “The Mother State and Her Weaker Children.”²¹ Men were active in social work and disproportionately represented

¹⁹ For voluminous evidence of these government-civic connections, see Fitzpatrick, *Endless Crusade*; Susan Ware, *Partner and I: Molly Dewson, Feminism, and New Deal Politics* (New Haven, Conn., 1987); Gordon, “Black and White Visions of Welfare”; Shelby M. Harrison, “Social Surveys,” in *Social Work Year Book 1929*, Fred S. Hall and Mabel B. Ellis, eds. (New York, 1930), 431–34; Molly Ladd-Taylor, “Mother-Work,” forthcoming book, typescript in my possession, chap. 4, 20–42.

²⁰ For example, the first chapter is titled “Home-making as a Social Art.” It begins: “Let us imagine a progressive woman in a village or town where the houses are bare, untidy, and ugly. What can she do to communicate her higher ideals? First, she can . . .”; Charles Richmond Henderson, *The Social Spirit in America*, 2d edn. (Chicago, 1901), 37.

²¹ Alexander Johnson, *Proceedings, National Conference of Charities and Correction* (NCCC), 1897, Isabel C. Barrows, ed. (Boston, 1898), 1–13.

in its leadership, but the field nevertheless carried a distinctly feminine identity.

ALTHOUGH SOCIAL WORK INVOLVED BOTH MEN AND WOMEN, with the women a dominant influence, another type of social welfare thinking developed—social insurance—as almost exclusively male. In what follows, these gendered visions will be explored in the programs proposed, in the rhetoric of the problem of poverty, in the fundamental principles on which welfare philosophies rested, and in research methods, disciplines, and epistemology. The personal and social biographies of the welfare activists will reveal how their class, race, professional, and familial circumstances, for example, contributed to forming their welfare visions and strategies.

First, a short detour is necessary to understand some of the language of welfare in the first half of the twentieth century. Distinctions between social work, social welfare, and social insurance developed within this period, but the three terms were used inconsistently and generically. “Social work” in particular remained an umbrella term, encompassing not only a developing profession but also a range of “helping” activities, voluntary and paid, private and public, relief and reform. This unity began eroding most visibly with the development of settlement houses in the 1890s, and fissures widened in the second decade of the twentieth century, as social workers built professional schools, devoted themselves to establishing their professional status, and separated themselves from reform activism. Nevertheless, well into the 1930s, the term “social work” continued to be used in its wider, inclusive meaning.²² This meaning was gendered. Every woman in the welfare network I identified considered herself a social worker, largely because the term described a unity women felt among employed and volunteer activists. While some of the men considered themselves social workers, they more often defined themselves by their vocation and not by their volunteer work. In 1931, Amy Maher, chair of the Ohio Council on Women in Industry, wrote Mary Dewson of the Democratic Party, reporting her positive reaction to a brief that women from the National Consumers’ League had written: “Its a great piece of work . . . and as feminists we’re proud of it, and as social workers, and as litterateurs!!” (She added, “also as college grads,” revealing how special that status still made women feel.)²³

Within this generic meaning of the term, there was a network of “social workers” who, throughout these years, shared a welfare program. Historians have often pointed out that this program expressed the social workers’ privileged class position. Accepting a community obligation to help the poor, most social workers

²² Failing to recognize this social work continuum, Donald Brieland in “The Hull-House Tradition and the Contemporary Social Worker: Was Jane Addams Really a Social Worker?” *Social Work*, 35 (March 1990): 134–38, is mistaken in his denial that Addams was a “social worker.”

²³ Amy Maher to Mary Dewson, September 6, 1931?, Dewson MSS, Box 2, folder 17, The Arthur and Elizabeth Schlesinger Library on the History of Women in America, Radcliffe College, Cambridge, Massachusetts. (“Sept. 6” is in Maher’s writing, and “1931?” was added later, probably by Dewson going through her own papers.) For a contemporary definition, see the *Encyclopedia of the Social Sciences*, Edwin R. A. Seligman, ed., 15 vols. (New York, 1931–35), entry for “public welfare,” 12: 687, by social work education leader Edward Lindeman.

believed that aid to the poor must avoid making their lot too easy and should never become more lucrative than the lowest-paid wage labor; this view was known as the “least eligibility” principle from the English Poor Law Reform of 1834. Social workers usually distinguished between the deserving and the undeserving poor and felt it important to treat them differently. The undeserving could threaten the entire social order by their failure to internalize a work ethic, and social workers worried about the potential of their own helping activity to worsen that shiftlessness. The most important word in the social work-vocabulary during the nineteenth century had been “pauperization,” which is what happened when the poor allegedly lost their work ethic and began to expect handouts. From this concern stemmed the conviction that good social work required not only economic relief but also moral reform; without it, poverty would become pauperism. Belle Israels, a member of the social work network, remarked in 1908, “Poverty easily drags the poor man down, weakens him physically, diminishes his moral resistance, makes him less valuable as a working force, and frequently *leads to* [my emphasis] lack of employment, as at every crisis or industrial depression the mediocre working men and women are the first to be dismissed.” She conceived of the problem as complex, multilayered, and self-perpetuating: being poor can make a person grow poorer still. She went on, “In individual cases it is often difficult to determine if poverty is the result of idleness, or idleness the result of poverty.”²⁴ Aid to the poor was always to be accompanied by guidance toward rehabilitation or at least maintenance of “good standards” of home life. In the early twentieth century, the emphasis on pauperization was criticized, and by the 1920s most social workers considered environment the primary cause of social maladjustment, some pinpointing economic distress and others individual problems. But most social workers remained committed to treating the individual who was damaged by the environment, even if they were less likely to refer to this damage as pauperization. They agreed that welfare required both screening out the undeserving recipients and personal supervision of the deserving.²⁵

MALE INFLUENCE ON WELFARE THOUGHT increased during the twentieth century, not through a critique of this female-influenced social work program but through a new concept, introduced at the turn of the century, social insurance. Its basic principles were government provision, based on compulsory participation and automatic (not means or morals-tested) benefits for covered groups. Indeed, social insurance programs were not exclusively directed at the poor; one of their selling points was that they benefited all classes.²⁶ While the social workers focused on treating poverty so as to prevent pauperism, the proponents of social insurance aimed to prevent poverty itself. Social insurance could achieve this end,

²⁴ Belle Israels [Moskowitz], “Poverty and Insurance for the Unemployed,” *Charities and the Commons*, 20 (June 6, 1908): 343–37.

²⁵ Judith Leavitt pointed out to me in a private conversation that the public health discourse had by this time shifted away from pauperism, because this kind of welfare required universal programs, such as municipal sewers and water.

²⁶ See Charles Richmond Henderson, “The Logic of Social Insurance,” *Annals of the American Academy of Political and Social Science*, 33 (March 1909): 265–77.

they believed, by aiding workers as soon as there was a loss of earnings, with, for instance, workmen's compensation, or by providing incentives for employers to maintain steady employment and safe working conditions, as, for example, through the use of tax incentives. They had great confidence that poverty could be prevented.²⁷ Richard Neustadt, who moved from settlement work, within the social work tradition, to an early advocacy of social insurance, considered the view that poverty "is ultimately preventable [to be] a fundamental doctrine of democracy, an axiom of civilization."²⁸ Proponents of this approach did not concentrate on perfecting individuals and devoted little attention to character.

While social work ideas came from men and women, social insurance thought was almost exclusively male. Ideas about social insurance came to the United States from Europe with male academics educated abroad, particularly in Germany, and opponents were quick to brand it un-American, a German plot.²⁹ The first to begin popularizing German social insurance ideas in the United States was John Graham Brooks, encouraged by Commissioner of Labor Carroll Wright in the 1890s. In 1902, Charles Henderson of the University of Chicago sociology department induced the National Conference of Charities and Correction (NCCC)—the main national professional organization of social work—to appoint a commission to study social insurance. Soon, several states organized studies of European schemes, and in 1906 the American Association for Labor Legislation was organized to campaign for workman's compensation, health insurance, and, later, unemployment insurance.³⁰ Many proponents of social insurance were academics, as we have seen; Wilbur Cohen, recalling this history many years later, thought that social insurance ideas started among people in "key colleges and universities."³¹

Generally, women in social work supported social insurance but rarely participated in its formulation. The NCCC commission, for example, was all male despite the prominence of women in the organization. Some of the exceptions are illustrative. Elizabeth Brandeis Raushenbush, the daughter of Louis Brandeis, was frequently involved in social insurance efforts but primarily as a kind of assistant to her husband, Paul Raushenbush, a student of John Commons and author of the Wisconsin unemployment compensation law. During the New Deal, two academic economists, Eveline Burns and Barbara Nachtrieb Armstrong,

²⁷ See Frank Tucker, presidential address, "Social Justice," NCCC *Proceedings* (1913): 1–13.

²⁸ *Report of the New York State Commission on Relief for Widowed Mothers* (Albany, N.Y., 1914), 15.

²⁹ Mary O. Furner, *Advocacy and Objectivity: A Crisis in the Professionalization of American Social Science, 1865–1905* (Lexington, Ky., 1975), 49; Roy Lubove, "Economic Security and Social Conflict in America: The Early Twentieth Century," *Journal of Social History*, 1 (1967–68): 61–87 and 325–50. In sociology, all the leading figures before 1915 except George Vincent had done graduate work in Germany; Martin Bulmer, *The Chicago School of Sociology: Institutionalization, Diversity, and the Rise of Sociological Research* (Chicago, 1984), 38. On German influence, see Jurgen Herbst, *The German Historical School in American Scholarship: A Study in the Transfer of Culture* (Ithaca, N.Y., 1965); and Arthur Mann, "British Social Thought and American Reformers of the Progressive Era," *Mississippi Valley Historical Review*, 42 (March 1956): 675.

³⁰ John Graham Brooks, "Compulsory Insurance in Germany," *Fourth Special Report of the Commissioner of Labor* (Washington, D.C., 1893); Frank J. Bruno, *Trends in Social Work, 1874–1946* (New York, 1948), 157 and following; John R. Commons and A. J. Altmeyer, "Special Report XVI: The Health Insurance Movement in the United States," 1919 typescript, copy in Altmeyer MSS, Box 15, vol. 8, and Cohen MSS, Box 33, State Historical Society of Wisconsin, Madison.

³¹ Wilbur Cohen, Columbia University Oral History Collection, Interview, 35.

worked on Social Security; but these were the first of a younger generation to enter the field, outsiders to both the social work and the women's rights networks, and they were critical both of social insurance and social work approaches. Edna Bullock, editor of the *Debaters Handbook Series* during the Progressive Era, anthologized excerpts from the national discussions of a variety of social issues, and her volumes often featured the work of many female activists. Yet her book on "compulsory insurance" had but one piece by a woman, out of thirty-two articles, and it was the only article that mentioned poverty and the only article from a social work, as opposed to an academic, journal.³²

Gender was also embedded in the language of these welfare activists. I do not mean here to distinguish language from ideas but merely to highlight an aspect of welfare ideas. The social work approach to poverty—the concern with personal maladjustment—emerged in the language of the reformers' writings. They used narrative and cited actual cases far more often than the social insurance advocates did. Their stories were sometimes sentimental, and their audiences were different: social insurance advocates were more often writing academic texts or reports, while social workers were addressing the general public, hoping to move their listeners and readers and thereby persuade them. This female style was more sophisticated than naïve. In 1923, Julia Lathrop referred to her group's accustomed use of "sob stuff [and] high ideals afloat."³³ Twelve years later, when the women of the Children's Bureau were drafting *Aid to Dependent Children*, and Grace Abbott's colleagues suggested she was asking for too small an appropriation, Abbott responded that to increase their request they should focus not just on children but on crippled children.³⁴ This manipulative rhetoric, however, did not mean rank opportunism. Lathrop's defense of pity in 1912 is instructive: "pity is a rebel passion . . . it does not fear the forces of society but defies them . . . [It] often has ruthless and stern ways, but . . . at last it is the Kingdom of Heaven working within us. The justice of today is born of yesterday's pity . . . [T]his bureau [the Children's Bureau] . . . is an expression of the nation's sense of justice. It will need as perhaps no other bureau of the government will need, the continuance of the popular pity which demanded and secured it."³⁵ Moreover, because their stories were taken from actual cases, social workers often exhibited the complexity of thought that combats sentimentality. They avoided the dichotomies of good and evil, victim and brute, innocent and guilty, pure and polluted, that characterized some Victorian women's reform rhetoric. At its best, the distinction between poverty and pauperism was precisely about this complexity, because the concept of pauperism could be seen as trying to integrate what Richard Sennett has called the "hidden injuries of class." There were some stereotyped stories of saintly, pale, overworked widows, but there were also stories of child abuse and maternal negligence, wife beating and alcoholism, demoralization and dishonesty. Social workers' narratives included condescension and

³² Edna D. Bullock, ed., *Selected Articles on Compulsory Insurance* (Minneapolis, 1912).

³³ Julia Lathrop to Grace Abbott, June 24, 1923, Abbott MSS, Box 57, folder 9, quoted in Muncy, *Creating a Female Dominion*, 132 n. 33.

³⁴ Martha Eliot, Columbia University Oral History Collection, Interview, 46–48.

³⁵ Julia Lathrop, "The Children's Bureau," in *NCCC Proceedings* (1912): 30–33.

moralism, to be sure, but they were not wrong to recognize personal and intra-family problems among the poor.

By contrast, the social insurance rhetoric was more often abstract. When a “case” was used as an example (an infrequent device), it was invented, simplified, or hypothetical. William Hard, for example, perhaps closest to the women in his concerns, used a hypothetical longshoreman “Smith,” who was burned in an explosion of benzine, naphtha, and gasoline on a ship, and explained how he would have been taken care of had there been a social insurance system.³⁶ Social insurance writing usually addressed incidences of illness, injury, or death, costs of various systems, administrative arrangements, or the impact of insurance on economic incentives. These choices of topic grew in part from the goal of persuading politicians and scholars of a new method of distribution of provision, while the social workers were defending a traditional form of aid. Social insurance abstractions did not tend toward the philosophical; very little ethical argumentation or grounding in social or political theory appeared. Writers spoke the language of actuarial computation and administration and argued from a logic that assumed profit motives as the only motives (despite, ironically, the socialist affiliations of several prominent social insurance advocates). Altruism was not only absent but was implicitly denied force as a social motive.³⁷

WHEN WELFARE ADVOCATES SOUGHT TO DERIVE THEIR ARGUMENTS from fundamental principles, the relevance of gender appeared again, albeit less dramatically. Men more often made rights claims. Frederick Wines spoke of a “natural right” to relief as early as 1883.³⁸ In *The Scientific Spirit and Social Work* (1920), Arthur James Todd began with the chapter “Natural Rights and Social Wrongs” and found the origins of modern social work not in the church, the peasant moral economy, the paternal bond between lord and peasant, or anything of the English tradition, but in *The Declaration of the Rights of Man* and Tom Paine. “There is no forcing of analogies or misreading of history when I say that *modern social-reform movements and social work represent a series of concrete attempts to define and redefine the Rights of Man* [emphasis in original].”³⁹ Prefiguring Franklin Roosevelt, Todd listed some new “rights”: to a decent income, to organize for economic protection (unions), to leisure, to education, to recreation, to health (including sanitation, preventive hygiene, protection from impure and adulterated food), to decent

³⁶ William Hard, “Pensioners of Peace,” in Bullock, *Selected Articles on Compulsory Insurance*, 118–41. Of course, it is possible that “Smith” was the pseudonym for a real person, but if so, my point still holds, for Hard was not interested in the actual facts of his life, only in the hypothetical situation of Smith under public insurance.

³⁷ See, for example, Paul Douglas, “Family Allowance System as a Protector of Children,” *Annals of the American Academy of Political and Social Science*, 121 (September 1925): 16–24; Abraham Epstein, *Insecurity: A Challenge to America* (1933; rpt. edn., New York, 1968); Abraham Epstein, *The Challenge of the Aged*, rev. 2d edn. (New York, 1928); I. M. Rubinow, *Social Insurance with Special Reference to American Conditions* (New York, 1913); I. M. Rubinow, ed., *The Care of the Aged: Proceedings of the Deutsch Foundation Conference* (Chicago, 1931); I. M. Rubinow, *The Quest for Security* (New York, 1934).

³⁸ Frederick Wines, “Laws of Settlement and the Right to Public Relief,” in *NCCC Proceedings* (1898): 223.

³⁹ Arthur James Todd, *The Scientific Spirit and Social Work* (New York, 1920), 2.

habitation, to a childhood “untainted by unnecessary and preventable diseases or degeneracies” (eugenics), and women’s rights.⁴⁰

Women often used the rhetoric of rights, too. Florence Kelley, in *Some Ethical Gains through Legislation* (1905), spoke of the right to childhood, to leisure, and the rights of purchasers. Kelley even found constitutional grounds for these: “the right to childhood . . . follows from the existence of the Republic.”⁴¹ But women more often used this rhetoric in combination with the notion that needs themselves were a claim on the polity. They suggested that fulfilling human needs was necessary to the social order and that needs were more numerous and complex than the merely biological. The rhetoric of needs was developed particularly strongly in the settlements, where leaders spoke of learning from the poor about their needs.⁴² The protracted struggle against child labor was a decisive factor in spreading and establishing an authoritative discourse about the needs of others, as when social workers argued that children “needed” play and companionship and spoke of “meeting” children’s needs.⁴³

An important aspect of social work—casework—was based on ascertaining the individual and social needs of clients. This is one of the ways in which casework skills could be said to be feminine, involving attentiveness, empathy, asking the right questions. Bertha Reynolds came to believe that casework was defined by “perceptiveness regarding needs.”⁴⁴ The variability of needs did not seem to these social workers to make determinations less exact; casework discourse sought to make needs scientific through empirical studies of the cost of living and the making of family budgets.⁴⁵ Social insurance plans, by contrast, required no discretionary ascertainment of need.

From the first infant mortality studies of the Children’s Bureau to the programs of the New Deal, the rhetoric of needs continued to be influential. At the end of the New Deal and particularly during World War II, left-of-center social workers moved toward integrating needs and rights principles.⁴⁶ An influential example,

⁴⁰ Todd, *Scientific Spirit and Social Work*, 9–14.

⁴¹ Florence Kelley, *Some Ethical Gains through Legislation* (New York, 1905), 3. Kelley’s rights discourse must be understood in the light of her Marxist education, for she frequently used the concept of exploitation and in general could be said to be postulating a right to freedom from exploitation.

⁴² Julia Lathrop in *NCCC Proceedings* (1894): 313–19; Lillian Wald, “Nurses in ‘Settlement’ Work,” in *NCCC Proceedings* (1895): 264–65.

⁴³ Henry S. Curtis, “The Playground,” 278–86, R. R. Reeder, “Study of the Child from the Institutional Standpoint,” 265–73, Graham Taylor, “Discussion of Playgrounds and Fresh Air Movement,” 294–95, 298, all in *NCCC Proceedings*, 1907, Alexander Johnson, ed. (Indianapolis, 1907); Owen R. Lovejoy, “The National Child Labor Movement,” in *ibid.*, 1910, Alexander Johnson, ed. (Fort Wayne, Ind., 1910), 232–35. An interesting example of the move from rights to needs is in Ora Pendleton, “A Decade of Experience in Adoption,” *Annals of the American Academy of Political and Social Science*, 212 (Philadelphia, 1940), 193, referring to the conflict between the “right” to confidentiality and the “need” of the adopted for knowledge about their birth parents. Edith Abbott and Sophonisba Breckinridge postulated such needs in their study *The Delinquent Child and the Home* (New York, 1912). Another historian of this women’s network, Molly Ladd-Taylor, also observed women’s infrequent use of rights rhetoric, in her “Mother-Work,” typescript chap. 4, p. 8.

⁴⁴ Bertha Reynolds, *An Uncharted Journey: Fifty Years of Growth in Social Work* (New York, 1963), 146.

⁴⁵ See Sidney E. Zimbalist, *Historic Themes and Landmarks in Social Welfare Research* (New York, 1977), chap. 4.

⁴⁶ William H. Simon, “The Invention and Reinvention of Welfare Rights,” *Maryland Law Review*,

and perhaps the ultimate defense of the women's social work tradition, was Charlotte Towle's *Common Human Needs*, published in 1945, when the ascendancy of the social insurance programs within the Social Security Act already threatened to confirm casework's stigmatizing effects on its recipients. Towle tried to unify the two rhetorics of rights and needs, arguing that there was a right to have one's needs met. "[P]ublic assistance services [the general name for relief accompanied by casework] achieve their broad social purpose only when those who administer them understand the significant principles which make for sound individualization in a program based on right . . . understanding of common human needs and . . . basic principles of human behavior." During the depression, "want and fear became the base for a progressive curtailment of man's freedom. Today, as repeatedly throughout history, this basic want and fear have engendered hostile feelings which, in turn, have pitted man against man and prompted him to use his scientific enlightenment in that wholesale destruction of life and property which now threatens not only the realization of his social goals but also his very survival."⁴⁷

Discourse about needs has been criticized because defining someone else's needs is a process open to manipulation. In social work, when "needs" are defined by professional experts, the client's own expression of need may be silenced, and the client may lose access to defending her claims through the adversary proceedings that adjudicate "rights." The practice of making family budgets allowed social workers to decide what clients needed economically; then, with psychiatric influence, social workers became authorities on what their clients needed spiritually as well. (The appropriation of Freud by social work often suppressed not only the discipline of nondirective, listening therapy that the orthodox psychoanalysts used but also the critique of "adjustment"—conformity—that Freudian thought contains.⁴⁸) The use of needs rhetoric as a means of control and even domination fit the skills of women in social work. Social insurance provided no individualized definition of a client; but casework defined a client as multiply needy, and this definition gave caseworkers a position of power and authority not easily reached at the time by women outside their families.

Egalitarian empathy is always difficult to achieve between caseworker and client. For these social workers, distance was created not only by casework theory, by their sense of class, religious, and racial or ethnic superiority, but also by their own family situations, in which they rarely combined earning and child rearing. Moreover, for caseworkers freed by their relatively privileged economic position from the necessity of marriage to a greater degree than poor women, being single was in many cases the condition that allowed their activism or career. Their singleness might logically have made caseworkers critical of the family-wage

44 (1985): 1–37; and Simon, "Rights and Redistribution in the Welfare System," *Stanford Law Review*, 38 (July 1986): 1430–1516.

⁴⁷ Charlotte Towle, *Common Human Needs: An Interpretation for Staff in Public Assistance Agencies* (Washington, D.C., 1945), vii, 1–2.

⁴⁸ Agnes Heller discussed this problem in several places, notably in "Can 'True' and 'False' Needs be Posited?" in her study *The Power of Shame: A Rational Perspective* (London, 1985); Russell Jacoby, *Social Amnesia: A Critique of Conformist Psychology from Adler to Laing* (Boston, 1975).

norm, but it did not do so. Their experience of singleness fit the class distance they felt from their clients and did not lead them to contradict the prevailing premises that children and women needed breadwinner husbands, that children needed full-time mothers, that women should choose between family and career.⁴⁹ Certainly, the premise of the social work mainstream during the period 1890 to 1935 (with the important exception of the social work rank-and-file movement during the depression) was that welfare clients could not define their own needs properly without professional assistance. In the depression, the social work Left, as represented in the rank-and-file unionizing drive and the journal *Social Work Today*, criticized the rhetoric of needs for its inegalitarian implications and began to reclaim the rhetoric of rights. Grace Coyle was typical of this tendency in her call to social workers to defend clients' rights as well as their needs—their right to organize, to social security, and to free speech.⁵⁰

The development of needs talk coincided with the rise of the “social,” as Hannah Arendt identified it, a realm distinct from the private (as in family or friendship), from the state, but also from the economic. In this realm arose a range of “wants” that could be classified as needs, from the material to the emotional, from telephones to “fulfillment.” Arendt, in this respect following a Frankfurt School orientation, saw the social as inevitably oppressive, a space being defined even as it was filled with the subtle forms of oppression that characterize advanced capitalist culture—advertising, market research, and opinion polls, “pop psych,” state and other coercive intervention into the private realm.⁵¹

Arendt's critique of the language of needs misses much, owing particularly to the absence of a gender analysis. Since needs talk often involves bringing into public a previously private discourse, such as on bodily and psychological matters, it has often been a feminine discourse, constructed by those who take responsibility for the quality of the private. Needs language was often brought into political debate by women. When women argued for access to public citizenship rights, such as the voting rolls or juries, they often argued from needs—of the poor, of the children, of the city dwellers. Arendt saw the social only as “one-dimensional space wholly under the sway of administration and instrumental reason.”⁵² But needs discourse has been intensely argumentative. The rhetoric of needs has been mobilized fervently and frequently in welfare activism: in campaigns against child abuse and neglect and changing prescriptions about what children “need,” in pressures to raise the minimum provision for the poor in conformity with new social needs, such as telephones, in the as-yet-failed

⁴⁹ To appreciate properly the progressive content of their family-wage position, it is useful to distinguish their version of the family-wage system—that women should be free to choose between family and public life—from the conservative version, which insisted that all women belonged in the home.

⁵⁰ Grace Coyle, “Social Workers and Social Action,” *Survey*, 73 (May 1937): 138–39; a few other examples among many include Bertha Reynolds, “Whom Do Social Workers Serve?” *Social Work Today*, 2 (May 1935): 5–7, 34; “Case Work Notebook,” *Social Work Today*, 6 (February 1939): 21–22.

⁵¹ Hannah Arendt, *The Human Condition: A Study of the Central Dilemmas Facing Modern Man* (Chicago, 1958), esp. chap. 2, sect. 9, “The Social and the Private.”

⁵² For this quotation and my view of needs talk, I am indebted to Nancy Fraser's “Struggle over Needs: Outline of a Socialist-Feminist Critical Theory of Late-Capitalist Political Culture,” in Gordon, ed., *Women, the State and Welfare*, 223, n. 12.

arguments for public medical provision for health care. Needs talk was sometimes turned introspective and made subversive, notably by settlement women. One stunning use was Jane Addams's insistence that settlement work fulfilled needs for the privileged women who did it, needs that she considered at once spiritual and "primordial."⁵³ The language of needs is not inherently dominating; it seems rather that needs and rights rhetoric represented in this case two different kinds of complex discourse, each containing authoritative and democratic potential.

The gender difference must not be overstated. Most political activists used a mixture of needs and rights rhetoric, as did Charlotte Towle or that rhetorical master, Franklin Roosevelt, in his speech "Freedom from Want." On balance, however, there is a gender division in this rhetoric and its assumptions, as well as class and race meanings. The rights legacy had been male traditionally, and its more inclusive republican version became overshadowed in the last two centuries by the possessive-individualist version, resting on forms of power—economic and political—from which most women and men were excluded. The authority of this approach to rights was the irrevocability and universality of law and contract. By contrast, the needs tradition called up a female experiential and discursive realm of nurturance and of authority through the power to give. The oppositional use of needs rhetoric represented similarly the bringing into public of a private logic, in the way in which family members ask for help because they need it, not because they deserve it or believe it is their share.⁵⁴

THE ORGANIZATION OF OCCUPATIONAL DISCIPLINES and methods through which welfare thought was expressed and shaped presents another gender orientation. Social insurance was mainly an academic stream of thought, and many of its most prominent proponents were academics: for example, Arthur Altmeyer, Wilbur Cohen, John Commons, Richard Ely, Isidore Falk, Abraham Epstein, Charles Henderson, Robert Hunter, John Kingsbury, Paul Raushenbush, Edgar Sydenstricker, and Edwin Witte. The men's primary method of agitating for social insurance, at least before the New Deal, was academic writing and publishing. By the 1920s, most of the Jews in this network (eleven out of fifteen) had found academic positions, despite anti-Semitism. Indeed, the academic profession and the social insurance reform network represented a means of upward mobility for the poor and foreign-born Jews in the group. Social insurance ideas were consonant with the European social democracy that had been the legacy of many of these Jews.

By contrast, the few women who were academics were usually only marginally so. Of the ten female welfare activists in academe, five taught social work, one was the president of a "Seven Sisters" women's college, and four taught traditionally academic subjects. The women primarily involved themselves in speaking, lobbying, and organizational work. The men they were closest to were the minority of

⁵³ This essay, "The Subjective Necessity of Social Settlements," originally delivered in 1892, was printed in Jane Addams, *Twenty Years at Hull-House* (1910; rpt. edn., Urbana, Ill., 1990), see esp. 69.

⁵⁴ I am here indebted to the work of Lisa Peattie and Martin Rein, *Women's Claims: A Study in Political Economy* (New York, 1983).

nonacademics with whom they worked in civic organizations, such as John Andrews, for many years head of the American Association for Labor Legislation, and Alexander McKelway of the National Committee on Child Labor. Yet the women also felt comfortable and confident with male academics, many of them having gone to the best schools.

The sex segregation of the social insurance approach was not due to women's poorer academic performance. Many of these women had been stellar college students who found themselves without career opportunity upon graduation or even after receiving doctorates. In this academically ambitious generation, nearly one-third of women earning college degrees between 1868 and 1898 went on to do graduate work, and many earned doctorates. However, of nine women who earned doctorates in the social sciences during the University of Chicago's first fifteen years, none obtained a faculty appointment, while two-thirds of the male Ph.D.s did.⁵⁵ Despite their nonacademic orientation, female reformers in the late nineteenth century had been prominent among those who envisioned the application of expertise (science) to society. As Franklin Sanborn of the American Social Science Association remarked in 1874, "The work of social science is literally woman's work"; it was the feminine side of political economy.⁵⁶ Jane Addams wanted to rationalize even children's recreation. "This stupid experiment of organizing work and failing to organize play,"⁵⁷ she wrote. Helen Sumner Woodbury called for the application of "scientific methods of invention and experiment . . . to political and economic affairs" and for taking up the science of the "production . . . of happiness."⁵⁸ Three decades later, Charlotte Towle called for extending the "critical, empirical attitude of the natural sciences . . . to the study of personality."⁵⁹

In the 1930s, members of the social work network continued Progressive traditions of statism and belief in expertise.⁶⁰ They favored coercive measures, such as housing codes, wages and hours regulation, health and safety regulation, food and drug testing and labeling. Despite men's greater achievement of professional status, women had at least an equal commitment to authoritative expertise. They sought professionalization of social work and scientific methods of public administration. The Children's and Women's Bureaus considered themselves pioneers in the incorporation of expertise and meritocratic principles into government. As local and state governments inaugurated welfare programs, after about 1910, women in these agencies were among the foremost proponents of non-patronage hiring and professional standards. Today, we might see an

⁵⁵ Fitzpatrick, *Endless Crusade*, 13: 72–75.

⁵⁶ Sanborn quoted in William Leach, *True Love and Perfect Union: The Feminist Reform of Sex and Society* (New York, 1980), 316; and in Thomas Haskell, *The Emergence of Professional Social Science: The American Social Science Association and the Nineteenth-Century Crisis of Authority* (Urbana, Ill., 1977), 137.

⁵⁷ Jane Addams, *The Spirit of Youth and the City Streets* (New York, 1909), 6. On Addams's scientific method, see Donna L. Franklin, "Mary Richmond and Jane Addams: From Moral Certainty to Rational Inquiry in Social Work Practice," *Social Service Review*, 60 (December 1986): 504–25.

⁵⁸ Helen Sumner Woodbury, "A Political Engineer," in Woodbury MSS, Box 5, folder 3, n.d., probably 1918, Wisconsin Historical Society.

⁵⁹ Towle, *Common Human Needs*, 3.

⁶⁰ Roy Lubove's characterization of this women's tradition as quintessentially voluntarist is misleading on this point; Lubove, "Economic Security and Social Conflict in America"; Roy Lubove, *The Struggle for Social Security 1900–1935* (Cambridge, Mass., 1968).

inconsistency between their commitment to civil service and their use of an “old girl network” to bring women into jobs, but this is not the way they saw it: they believed they needed such a network to help highly qualified women overcome discriminatory practices.⁶¹

Moreover, social workers in this period were continually anxious about their status as professionals. In 1915 at the annual National Conference of Charities and Correction, Abraham Flexner declared that social work lacked the characteristics that would allow it ever to become a profession, a critique repeated by Maurice Karpf in 1931.⁶² The defensive response to these insults to social work should not obscure the gender issues here. (Another occupation that has been marked by equal anxiety about its professional status is nursing.) This network of women had a triple burden: proving that women, social workers, and government employees could be professionals.⁶³

The sexual division of labor among welfare proponents was more than a matter of discrimination, for at issue is not just what the women were kept from doing but what they did do. This division of labor by sex, in fact, affected the structure and content of our welfare programs and, furthermore, of the modern social science disciplines, especially sociology. At the turn of the century, there was little distinction between sociology and the social research wing of social work, or between social reform and applied sociology. Some who worked for the charities called themselves sociologists, while at the turn of the century Lester Ward acknowledged, albeit critically, that most people identified sociology with philanthropy.⁶⁴ The same motives that led women into social work were drawing them in large numbers into graduate sociology and, to a slightly lesser extent, economics departments, which were rapidly expanding at this time. The understanding of their professors was that these female students would do “applied” work, research rather than teaching, and research about contemporary social problems for institutes, government investigations, and the private sector, not for universities. Ida Merriam, considered a brilliant young economist, upon earning her doctorate in 1928 from Brookings⁶⁵ had three job offers: from the *Encyclopedia of the Social Sciences*, from the Committee on the Costs of Medical Care under director Isidore Falk (a leading social insurance proponent), and from Eastman Kodak. In her memoir, Merriam recalled her sense of resignation at not being considered for an academic job.⁶⁶ Edwin Witte much admired Elizabeth Wilson’s thesis on “Distribution of Disability Costs” and recommended that she seek a job

⁶¹ On patronage, see Muncy, *Creating a Female Dominion*, 155–56.

⁶² Abraham Flexner, “Is Social Work a Profession?” *NCCC Proceedings* (1915): 576–90; Maurice J. Karpf, *The Scientific Basis of Social Work: A Study in Family Case Work* (New York, 1931), 353.

⁶³ Nina Toren, *Social Work: The Case of a Semi-Profession* (Beverly Hills, Calif., 1972).

⁶⁴ Furner, *Advocacy and Objectivity*, 305, 310; Lester Ward, “Contemporary Sociology,” *American Journal of Sociology*, 7 (1901–02): 477, quoted in Anthony R. Oberschall, “The Institutionalization of American Sociology,” in *The Establishment of Empirical Sociology*, Oberschall, ed. (New York, 1972), 209–10. The mainstream of social work, however, as represented by Mary Richmond, resisted the academic aspirations of social workers and valued casework experience over social data.

⁶⁵ The Brookings Institution at that time ran the Brookings Graduate School of Economics and Government.

⁶⁶ Ida Merriam, Women in Federal Government Oral History Project, Interview, p. 18, Schlesinger Library. On women’s exclusion from academic jobs, see also Rosalind Rosenberg, *Beyond Separate Spheres: Intellectual Roots of Modern Feminism* (New Haven, Conn., 1982), 239.

in government, with the Social Security Board.⁶⁷ It is arguable that the development of the entire modern field of social work and the social survey was shaped by the refusal of the University of Chicago sociology department to hire Edith Abbott or Sophonisba Breckinridge; this refusal impelled the women to build the School of Social Service Administration, which dominated research in social work for many decades.⁶⁸

We must not take as given or predetermined the definitional boundaries of "academic/nonacademic" and "basic/applied," any more than we accept as eternal the notion that women did not fit in as university professors. The definition of "applied" research that excluded it from universities has not been stable; today, work under that definition is commonplace and respected in social science departments. Moreover, women were active in the infancy of quantitative social scholarship. Until World War I, social science quantitative work was mainly developed outside universities, by reformers, and women were prominent among them.⁶⁹ In 1878, when the tenth federal census was being planned and monies appropriated for it, the Association for the Advancement of Women was one of the few groups that offered critiques of the 1870 census and offered proposals for 1880.⁷⁰ Massachusetts statistical officer Carroll Wright recognized and supported the survey work being done by female investigators and the importance of the issues they raised: he backed Annie Howes's study of the health of female college graduates in the early 1880s, published *The Working Girls of Boston* in 1884, and sponsored a national study of working women in 1888. Women's contributions in this field were further marked by Helen Campbell's quantitative monograph *Women Wage-Earners* (1893) and Lucy Maynard Salmon's study of domestic servants in the *Journal of the American Statistical Association* (1892). When Wright persuaded Congress to fund investigations into urban slum conditions in the 1890s, he turned to the settlement women of Hull House to conduct the Chicago survey.⁷¹ Many contemporaries and modern scholars consider the result, *Hull-House Maps and Papers* (1895), one of the most influential social surveys, joined a bit later by W. E. B. Du Bois's *Philadelphia Negro* (1899) and Atlanta University Studies (1898–1913), and the Pittsburgh Survey (1914), also done mainly by women.⁷² Florence Kelley, through her work in Chicago as an Illinois factory

⁶⁷ Edwin Witte to Elizabeth Wilson, January 18, 1935, Witte MSS, Box 33, Wisconsin State Historical Society.

⁶⁸ Bulmer, *Chicago School of Sociology*, 39, 68; Fitzpatrick, *Endless Crusade*, 212; Muncy, *Creating a Female Dominion*, chap. 3.

⁶⁹ Oberschall, "Institutionalization of American Sociology," 215–16.

⁷⁰ Nancy Folbre and Marjorie Abel, "Women's Work and Women's Households: Gender Bias in the U.S. Census," *Social Research*, 56 (1989): 545–69.

⁷¹ On Wright and on the general history of social surveys, see David Ward, *Poverty, Ethnicity, and the American City, 1840–1925: Changing Conceptions of the Slum and the Ghetto* (Cambridge, 1989), 53 and following, 123–28; Margo Anderson, "The History of Women and the History of Statistics," paper presented at Berkshire Women's History Conference, June 1990, typescript, 24 and following; Mary O. Furner, "Knowing Capitalism: Public Investigation and the Labor Question in the Long Progressive Era," in *The State and Economic Knowledge: The American and British Experiences*, Mary O. Furner and Barry Supple, eds. (Cambridge, 1990), 255–56.

⁷² Margaret Byington, Crystal Eastman, Elizabeth Butler, and Florence Lattimore did four of the six parts of the Pittsburgh Survey. *Hull-House Maps and Papers*, of which Florence Kelley was the main author, was published in a series edited by pro-labor economist Richard Ely of the University of Wisconsin. He had first discovered Kelley as the translator of Engels and an important New York

inspector, and later in developing projects funded by the Russell Sage Foundation, was one of the definers of modern standards for social research along with Wright and Du Bois. The women's network was influential in persuading the National Conference of Charities and Correction of the importance of social data and in establishing its first committee on statistics in 1905. Julia Lathrop argued, for example, that America's backwardness in the matter of social provision was a result of its public not being given "ascertained facts."⁷³ Kelley listed at length the issues on which information was lacking.⁷⁴ Their approach helped create the modern practice of conducting studies to serve as a basis for policy decisions.⁷⁵ Their belief in the power of data to persuade was of a piece with their rhetorical preference for the concrete, the specific. And their influence helped transform social work. Doing social surveys became so common that it could be called a social movement; at least 2,775 were completed by 1927.⁷⁶ An indicator of the growing importance of surveys was the renaming of *Charities and the Commons*, the key journal of the social work establishment, as *Survey* in 1909, newly supported by a grant from the Russell Sage Foundation.⁷⁷

By the Progressive Era, many women were acknowledged authorities in the discipline of "social statistics."⁷⁸ By the 1920s, the woman-led Children's Bureau in the Department of Labor was recognized as the federal government's leader in statistical studies.⁷⁹ The test for applicants to the Children's Bureau required outlining a statistical table from raw data and writing a plan for investigation of a social problem.⁸⁰ The bureau was the source of most depression-era data on

socialist intellectual before she came to Chicago. The study, addressed to "the constantly increasing body of sociological students," documented an area about a third of a square mile around Hull House. It detailed methods of investigation, defined its categories, and reprinted the schedules used. It also challenged the 1890 census. To cite but one example, the census had reported 5,426 employed children in Illinois, while Kelley and her crew identified 6,576 in 1894 in only five months of investigation. It featured graphically complex, color-coded maps (inspired by Charles Booth's maps of London) that offered visual representation of sociological conditions, including demography, migration patterns, ethnicity, wages, occupations, and housing conditions. Oberschall also sees *Hull-House Maps and Papers* as the originating work of the American social survey, in "Institutionalization of American Sociology," 216, as does Michael Gordon, "The Social Survey Movement and Sociology in the U.S.," *Social Problems*, 21 (1973): 290; and Robert E. Park, "The City as a Social Laboratory," in *Chicago: An Experiment in Social Science Research*, T. V. Smith and Leonard P. White, eds. (Chicago, 1929), 4.

⁷³ Julia Lathrop, "Institutional Records and Industrial Causes of Dependency," *NCCC Proceedings* (1910): 523.

⁷⁴ Florence Kelley, "What Our Official Statistics Do Not Tell," *NCCC Proceedings* (1910): 502–07.

⁷⁵ Shelby M. Harrison, *The Social Survey* (New York, 1931), 31.

⁷⁶ Allen Eaton and Shelby M. Harrison, *A Bibliography of Social Surveys* (New York, 1930).

⁷⁷ Sheila Slaughter and Edward T. Silva, "Looking Backwards: How Foundations Formulated Ideology in the Progressive Era," in Robert F. Arnove, ed., *Philanthropy and Cultural Imperialism: The Foundations at Home and Abroad* (Boston, 1980), 59.

⁷⁸ The early (1890–1930) social surveys were not exactly statistics in the modern sense: they did not use probability sampling, and they did little if any analysis of their data. These techniques were developed in survey research only in the late 1930s. See Bulmer, *Chicago School of Sociology*. Nevertheless, the term "statistics" is loosely used and was regularly applied to this early work. I have refrained from calling it statistical unless I am quoting, using instead the appellation "quantitative" for this mainly descriptive work.

⁷⁹ Zimbalist, *Historic Themes and Landmarks*, 193–95. For an example of the Children's Bureau's self-consciousness about this role, see Grace Abbott to Frances Perkins, memo, March 23, 1933, in Abbott MSS, Box 36, folder 13, Regenstein Library, University of Chicago.

⁸⁰ Muncy, *Creating a Female Dominion*, 91.

poverty: when better information on unemployment and wage rates was wanted, Secretary of Labor Frances Perkins turned to Clara Beyer of the Children's Bureau to set up the Bureau of Labor Standards and collect data.⁸¹

Within the developing field of sociology, women's overrepresentation in this kind of research was such that quantitative studies were sex-typed as female. Empirical research on contemporary social problems such as poverty, crime, and immigration was increasingly considered a branch of (female) social work while (male) sociological scholarship remained more theoretical and qualitative.

The story of the relationship between Hull House, arguably the most important enclave of the women's welfare network from 1890 to 1910, and the University of Chicago sociology department may serve to illustrate the growing division.⁸² Between these two centers of social research and scholarship there arose close cooperation and a clear division of labor. The first sociology faculty were from the social work, Christian reform tradition, such as Charles Henderson and Graham Taylor, and Dean of Women Marion Talbot, who also taught in the department during its first years. They greatly admired Jane Addams, who frequently lectured in university sociology classes, often enough that one historian described her as an "adjunct professor."⁸³ In 1895, the university tried to acquire Hull House, but the offer was rejected by Addams and Lathrop; they had several reasons for preferring autonomy, despite the value of durable economic support, one of which was the inevitable subordination that would be the settlement's fate inside such a male institution as a university in the 1890s. Accepting Hull House's autonomy, University of Chicago sociologists still viewed it as their "laboratory."⁸⁴ These sociologists considered quantitative work to be "applied" and preferred to subcontract it, so to speak, to lower-status workers—in this case, often the Hull House women, led particularly by pioneer social researcher Florence Kelley. After about 1915, quantitative work at the University of Chicago was deemphasized when sociologists such as W. I. Thomas, Robert Park, and George Herbert Mead developed their influential qualitative methods. (Ironically, W. F. Ogburn, hired at the University of Chicago in 1927, is often credited with initiating quantitative sociology.⁸⁵) Nothing illustrates the malleability of gender meanings

⁸¹ Meg McGavran Murray, "The Work Got Done: An Interview with Clara Mortenson Beyer," in *Face to Face: Fathers, Mothers, Masters, Monsters—Essays for a Nonsexist Future*, M. M. Murray, ed. (Westport, Conn., 1983), 203–32.

⁸² Steven J. Diner, "Department and Discipline: The Department of Sociology at the University of Chicago, 1892–1920," *Minerva*, 13 (Winter 1975): 514–53; Albert Hunter, "Why Chicago?" *American Behavioral Scientist*, 24 (November–December 1980): 215–27.

⁸³ Rosenberg, *Beyond Separate Spheres*, 33. Mary O. Furner also identified the importance of Hull House to early quantitative sociology in "Knowing Capitalism," 242.

⁸⁴ Mary Jo Deegan, *Jane Addams and the Men of the Chicago School, 1892–1918* (New Brunswick, N.J., 1988), 34–37; Rosenberg, *Beyond Separate Spheres*, 33; Hunter, "Why Chicago?" 215–27. Already in 1894, a University of Chicago sociology department brochure called settlement houses "'social observing stations' where students could 'establish scientific conclusions by use of evidence which actual experiment affords'"; Fitzpatrick, *Endless Crusade*, 41.

⁸⁵ Dorothy Ross drew a similar conclusion in *The Origins of American Social Science* (Cambridge, 1991), 226. Or consider this history: "Primarily under the stimulus of W. I. Thomas and Robert Park (1864–1944), sociologists at the University of Chicago formed in 1923 the Local Community Research Committee . . . which began a series of studies of the changing character of urban life and group relations"; Pauline V. Young, *Scientific Social Surveys and Research*, 4th edn. (New York, 1966), 44.

so well as the transition over the last century from a feminine association with statistical work to today's language about "hard data" and the masculine domination of the "hard" social sciences.⁸⁶

This gender reversal occurred mainly in the 1930s. As academic sociology then returned with enthusiasm to quantitative research, that work became detached from social action. Gradually, the social survey tradition, which had become part of social work, was separated from sociological survey research, which defined itself as disinterested.⁸⁷ Sociology, particularly at the University of Chicago, moved from closeness to coolness in its relation to social welfare, so that welfare research and social surveys were increasingly associated with the School of Civics and Philanthropy, renamed School of Social Service Administration in 1920 by Breckinridge and Abbott.⁸⁸

The divorce of social work from sociology underlay yet another gendered set of meanings, those concerning objectivity. The social survey movement had established the premise that welfare policy should be based on empirical data;⁸⁹ a corollary was that most social studies of the early twentieth century had as their purpose policy recommendations. A bibliography of social surveys refers to them as "Reports of Fact-finding Studies Made as a Basis for Social Action."⁹⁰ The notion that policy should rest on accurate data did not make the early social work researchers aspire to objectivity in the sense of disinterest or political neutrality.

The partisan use of social research characterized all welfare advocates, male and female, of the social insurance as well as the social work persuasion. They were closer to the nineteenth-century, often amateur, social scientists of the American Social Science Association, than to the twentieth-century, discipline-specialized academic social scientists. But, while the reform-related research tradition involved both men and women, the separation between academics and

⁸⁶ The important early role of women in developing quantitative social science represents an emendation to Rosenberg's study, *Beyond Separate Spheres*, which suggests that quantification was among the factors excluding women from academic scholarship (p. 241). A more recent article gets the situation entirely wrong. Howard Goldstein, in "The Knowledge Base of Social Work Practice: Theory, Wisdom, Analogue, or Art?" *Families in Society: The Journal of Contemporary Human Services*, 71 (January 1990): 32–43, distinguishes the male "scientific pursuit" strategy in early social work from the female "down-to-earth, humanistic," do-gooders. On the language of hard and soft, consider Michael Gordon's rendition of these early sociologists' "point of view that the survey provided the vehicle for the transformation of sociology from a 'soft' to a 'hard' discipline"; M. Gordon, "Social Survey Movement," 294.

⁸⁷ In the 1929 *Social Work Yearbook*, for example, there were separate entries on social surveys and social research. The latter entry considered that "how nearly such informal contributions and the numerous and popular reports, surveys, and social studies approach research rather than unsupported opinion depends upon the validity of the methods used." But the author's sixteen examples of good recent social research included only six by academics, ten by nonacademic reformers. See H. L. Lurie, "Social Research," in *Social Work Yearbook 1929*, Fred S. Hall and Mabel B. Ellis, eds. (New York, 1930), 415–20.

⁸⁸ Bulmer, *Chicago School of Sociology*, 39, 68; Hunter, "Why Chicago?"; Diner, "Department and Discipline."

⁸⁹ Although it marginalizes women's contributions, Thomas Haskell's interpretation of the meaning of the development of professional social science is consonant with mine on this point. He identifies professionalism as "a major cultural reform, a means of establishing authority so securely that the truth and its proponents might win the deference even of a mass public"; Haskell, *Emergence of Professional Social Science*, 65.

⁹⁰ Eaton and Harrison, *Bibliography of Social Surveys*.

advocates was primarily a male creation.⁹¹ The development toward “objective,” professional, disciplinary, technical social science has been narrated in such a determinist fashion, and as so inevitable, that the story is virtually teleological. As Mary Furner showed in her *Advocacy and Objectivity* (1975), the victory of the professionals in social science occurred through pitched political battles in which those committed to “objectivity” as an academic norm used their institutional power to shut out radicals and mavericks of various sorts.⁹² The role of women has been invisible in this history because they did not hold academic positions, but they were present in the social science debates nonetheless.

The social work researchers made no claim to producing value-free work. They believed that scholarship could be truthful *and* moral *and* partisan. Carroll Wright, for example, never maintained that statistics were neutral or inert with respect to social or political controversies. His early focus on “special” groups and “dependent classes” served his desire to demonstrate the need for state intervention; in publishing the 1890 census, he knew that his improved data on women’s employment, controlled as they were for “conjugal condition,” would challenge the myth of the family wage.⁹³ He, like his female allies, openly took stands on social and moral issues. Responding to the allegations that “shop girls” were immoral, for instance, he asserted that his study showed they were instead “honest, industrious and virtuous, and . . . making an heroic struggle against many obstacles, and in the face of peculiar temptations, to maintain reputable lives.”⁹⁴ As Agnes Sinclair Holbrook remarked in her introduction to *Hull-House Maps and Papers*, “The painful nature of minute investigation, and the personal impertinence of many of the questions asked, would be unendurable and unpardonable were it not for the conviction that the public conscience when roused must demand better surroundings for the most inert and long-suffering citizens of the commonwealth. Merely to state symptoms and go no farther would be idle; but to state symptoms in order to ascertain the nature of disease, and apply, it may be, its cure, is not only scientific, but in the highest sense humanitarian.”⁹⁵ Even Sophonisba Breckinridge and Edith Abbott, two of the most exclusively academic members of the women’s welfare network, insisted on purposeful research—data should be collected because of their contribution to reform—and required students to participate in reform activity.⁹⁶ Abbott said of her mentor Julia Lathrop that she rejected “the academic theory that social work

⁹¹ We cannot know if the lines would have been differently drawn had women been numerous in university faculties, but we must at least raise the question, as Ellen Fitzpatrick has done in her “Rethinking the Intellectual Origins of American Labor History,” *AHR*, 96 (April 1991): 427.

⁹² Mary Furner has observed the political implications of this development well but, at the time her book was written, neglected its gender dimensions. Ellen Fitzpatrick, in her fine study of four women of this group, saw them as representing a transitional phase between less and more advanced scholarship and took the victory of the male-model “nonpartisan” scholarship of the universities after the 1920s as unarguable and inevitable progress; Fitzpatrick, *Endless Crusade*, 66 *passim*. I would argue a less determinist version of this transition.

⁹³ See Anderson, “History of Women and the History of Statistics,” 23; Furner, “Knowing Capitalism,” 255–56.

⁹⁴ Quoted in Anderson, “History of Women and the History of Statistics,” 28.

⁹⁵ Residents of Hull House, *Hull-House Maps and Papers* (New York, 1895), 14.

⁹⁶ Muncy, *Creating a Female Dominion*, 82, 84–85.

could be 'scientific' only if it had no regard to the finding of socially useful results and no interest in the human beings whose lives were being studied."⁹⁷

At the turn of the century, no gender difference was visible on this topic, but the rise of academic social science soon created one. The 1909 White House Conference on Children produced an open disagreement about objectivity, with men and women often on opposite sides. Lillian Wald spoke for the unity of research and advocacy; Homer Folks and a male group insisted on their separation.⁹⁸ By the 1920s, male academic social scientists increasingly denied that their work was value-laden and considered themselves not reformers but consulting experts to reform.⁹⁹ By contrast, female social workers continued to maintain that their studies were intended not only to reveal sufferings and injustices but also to advocate government intervention—"welfare"—and to plead for social cooperation, not social conflict, as the basis for progress and order. Let us recall here their view of social work as a vocation that united study and action, accuracy and social purpose, individual help and social change.

FOR A NUMBER OF REASONS, one might have expected some convergence between the male and the female welfare discourse by the 1920s. First, academic social science insistence on "objectivity" contributed to a division between sociologists and social workers, and advocates of social insurance sided with the latter camp, because they were not disinterested scholars. "One would not be true to one's self or to one's conscience were one, after all the facts have been revealed, to speak calmly and 'impartially' of the injustice now meted out," Abraham Epstein wrote in 1928 (in a book he dedicated to New York Governor Franklin Roosevelt and Lieutenant-Governor Herbert Lehman).¹⁰⁰ Second, the rise of social insurance thinking shifted the male welfare discourse away from sociology toward economics, although the distinction was not particularly sharp at this time; this transition was accompanied by greater male interest in the utility of numbers. Third, academic economics became increasingly laissez-faire while the social insurance spokesmen wanted government intervention. But no convergence developed. Instead, the social insurance spokesmen grew closer to professional social science, and they tended to argue in non-ethical, technical terms (creating the discourse of today's poverty research and welfare policy¹⁰¹). The explanation for this apparent

⁹⁷ Jane Addams, *My Friend Julia Lathrop* (New York, 1935), 161, quoted in Ladd-Taylor, "Mother-Work," typescript, chap. 4, p. 28.

⁹⁸ Muncy, *Creating a Female Dominion*, 43–46.

⁹⁹ This was particularly characteristic of William Ogburn at the University of Chicago; he argued in his 1929 Presidential Address at the American Sociology Society that "sociology as a science is not interested in making the world a better place"; quoted in Oberschall, "Institutionalization of American Sociology," 242–43. For another gendered analysis of Ogburn's views on objectivity, see Barbara Laslett, "Unfeeling Knowledge: Emotion and Objectivity in the History of Sociology," *Sociological Forum*, 5 (1990): 413–33; and Laslett, "Biography as Historical Sociology: The Case of William Fielding Ogburn," *Theory and Society*, forthcoming; see also Leon Fink, "'Intellectuals' versus 'Workers': Academic Requirements and the Creation of Labor History," *AHR*, 96 (April 1991): 406; Harrison, *Social Survey*, 20–21.

¹⁰⁰ Epstein, *Challenge of the Aged*, ix.

¹⁰¹ On the technocratic nature of contemporary discussions of poverty and welfare, see Michael B. Katz, *The Undeserving Poor: From the War on Poverty to the War on Welfare* (New York, 1989).

paradox lies in the different strategies the two groups chose for eliminating poverty.

Both female social workers and male social insurance advocates were dissatisfied with relief programs and sought to promote prevention instead. But while social insurance advocates wanted to prevent poverty, social workers aimed to prevent pauperization. Poverty is an economic condition—lack of money—while pauperism is an internal, moral, or psychological problem. Pauperism might be caused by poverty, but not all poor people were paupers. We have seen that environmental causes of pauperism were emphasized in the early twentieth century, but the condition was remarkably similar to that of the nineteenth: a culture of poverty, or long-term dependency, marked by loss of work ethic, inability to accept industrial discipline, hopelessness, and sexual immorality—what is today called “underclass” behavior. Thus “prevention” in the social work tradition required environmental and character reform, not just economic tinkering. Gertrude Vaile emphasized the “rehabilitative forces,” such as “local churches, local schools, local health facilities, parks and libraries and social organizations and the daily job,” which could be brought to bear on individual cases.¹⁰² This fear of pauperization receded somewhat over time, and the social work Left in the 1930s became particularly critical of it. But the social work leadership retained it. Vaile was one of many who, even in the depths of the depression, feared that relief without casework would encourage clients to “lean.”¹⁰³

Just as social insurance advocates devised schemes aimed simultaneously at helping and preventing the need for help, social workers used casework to accomplish the same goals. The casework approach was individualized, as opposed to social insurance administration; often called “differential casework,” it required in-depth investigation of a “client’s” background, circumstances, and attitudes. Individualization was the essence of casework: its guiding principle was “to treat unequal things unequally.”¹⁰⁴ The very premise of casework was antibureaucratic, in the pure sense of bureaucracy, since it insisted on the worker’s discretion.¹⁰⁵ The caseworker was emphatic that money alone was not enough. The impersonal social insurance approach was anathema to social workers unless combined with other programs that provided casework.

But casework was also scientific in an important sense. It derived from the “scientific charity” movement but went beyond it, too. During the nineteenth century, charity workers had identified causes of poverty, which they then counted. As Lilian Brandt pointed out in 1906, “The method consisted in tabulating the opinions of a large number of charity workers as to what was the

¹⁰² Gertrude Vaile, in Herbert Elmer Mills, *College Women and the Social Sciences* (New York, 1934), 29–30.

¹⁰³ Vaile, in *College Women*, 26; Lilian Brandt, *An Impressionistic View of the Winter of 1930–31 in New York City, Based on Statements from Some 900 Social Workers and Public-Health Nurses* (New York, 1932), 23–24, 44–48.

¹⁰⁴ Mary Richmond, quoted in Roy Lubove, *The Professional Altruist: The Emergence of Social Work as a Career, 1880–1930* (Cambridge, Mass., 1965), 47–48.

¹⁰⁵ Kathy E. Ferguson, *The Feminist Case against Bureaucracy* (Philadelphia, 1984), argued that the female style is inherently anti-bureaucratic; this argument is similar to that made by Carol Gilligan that women have different moral approaches.

cause of poverty in a large number of individual cases . . . [This] meant reliance on opinions, not on facts; and . . . the burden of deciding whether it was intemperance, lack of work, unwise philanthropy, inefficiency or illness, in a given case, that brought the family to dependence, and the conviction arising that the decision could not be of much value, did much to make statistics in general hateful to charity workers."¹⁰⁶ These single-cause data were then used to separate out the deserving. A table of 28,000 cases investigated by the Charity Organization Societies in 1887 produced the following results.¹⁰⁷

Worthy of continuous relief	10.3 percent
Worthy of temporary relief	26.6
Need work rather than relief	40.4
Unworthy of relief	22.7

Scientific casework rejected such enumeration of simple, alternative, isolatable causes and insisted on the context of each recipient, which might include family, neighborhood, or the entire society. This new understanding did not equate science with single-factor, unambiguous answers.¹⁰⁸

One of the attractions of social insurance schemes was their rejection of this tradition of identifying the "deserving," thus insulating their recipients from stigma.¹⁰⁹ Yet many social insurance advocates understood the distinction between insurance and public assistance as ideological and political, and considered "insurance" a metaphor. All tax-funded public provision is in a sense insurance, to which all contribute in order to provide for those who happen to need help. Moreover, no common definition of insurance actually applies to all social insurance programs; for example, Unemployment Insurance (UI) is not usually based on workers' contributions, and few insurance programs rest on an actuarial relation between contributions and payments.¹¹⁰ "[T]he sooner we get away from our fine distinctions of insurance versus relief the better off we will be," Abraham Epstein argued in 1933.¹¹¹ The consensus for promoting the insurance idea was grounded in politics, not economic planning. By invoking the "apparent analogy with private insurance," the programs would become "acceptable to a society which was dominated by business ethics and which stressed individual economic responsibility," Eveline Burns maintained.¹¹² "Social insurance" was called "contributory" (although all tax monies are in fact contributions) and could therefore be made to seem "earned" in a way that the dole was not.¹¹³ Nothing about this

¹⁰⁶ Lilian Brandt, "Statistics of Dependent Families," *Proceedings, National Conference of Charities and Corrections*, 1906, Alexander Johnson, ed., 436, quoted in Zimbalist, *Historic Themes and Landmarks*, 43.

¹⁰⁷ Amos Griswold Warner, Stuart Alfred Queen, and Ernest Bouldin Harper, *American Charities and Social Work*, 4th edn. (New York, 1930), 45.

¹⁰⁸ See, for example, Margaret F. Byington, "Fifty Annual Surveys," *Survey*, 23 (1910): 972-77.

¹⁰⁹ Israels, "Poverty and Insurance," 343-47.

¹¹⁰ Burns, *American Social Security System*, 29.

¹¹¹ Cited by Daniel Nelson, *Unemployment Insurance: The American Experience, 1915-1935* (Madison, Wis., 1969), 195-96. See also Isaac Rubinow, *Social Insurance with Special Reference to American Conditions* (New York, 1913), 7; Eveline Burns, "Social Insurance in Evolution," *American Economic Review*, 34 (March 1944), supplement, pt. 2, p. 199.

¹¹² Burns, "Social Insurance in Evolution."

¹¹³ A different approach might have been to sell to the public an understanding of taxes as themselves insurance—contributory and producing "earned" benefits.

approach differed from that of the social work tradition. Edith Abbott argued passionately at the beginning of the New Deal for seizing the opportunity to create one universal program of public assistance without stigma.¹¹⁴

The irreducible conflict, the difference that ultimately made AFDC separate from the Social Security Act's social insurance program, came not from the social work rejection of insurance or universal plans of provision but from the social insurance rejection of casework. In the New Deal, Harry Hopkins, Roosevelt's main adviser on relief, referred to the social workers in the federal government (mainly women) as "pantry snoopers."¹¹⁵ What was it about casework that repelled the social insurance advocates? There is no evidence that proponents of social insurance were more generous or democratic in their class or cultural attitudes. Rather, since they were not mainly "front-line" social workers, actually meeting the poor, and since they did not direct their primary concern toward the very poor anyway, they ignored actual evidence of need for help other than financial, such as counseling or education. One cannot avoid wondering, also, if men considered the supervision inherent in casework more demeaning than did women.¹¹⁶

On the other hand, why were the social workers so committed to casework, and why did they fail to make social insurance proposals of their own? By the beginning of the depression, the casework approach was widely accepted and, among professionals, fundamental. Casework was, in some respects, the exact opposite of social insurance thinking: it meant assembling all the unique facts of a "case" and attempting to solve or ameliorate problems on the basis of many subtle as well as obvious characteristics of human individuals and groups. Casework thinking corresponds to stereotypes of the female approach: specific rather than universal, grounded rather than abstract, tailored rather than generalized. And casework represented, for this class of women, an integration of their impulse toward control with a female acculturation to help, and to help personally.

In this sense, casework played an ironic role in the outlook and contribution of social workers. Casework had arisen in the early twentieth century out of the desire to define an objective, professional method. In their struggle for professional status, social workers wanted to shed the practices and values of their legacy of friendly visiting: sentimentality, intuition, and individual kindness. In 1930, the leading historical text on social work argued that "helpful" relationships came in four types—"friendly, benevolent, commercial and professional."¹¹⁷ Indeed, some leaders longed to defeminize their field. When Stuart Alfred Queen wrote

¹¹⁴ Edith Abbott, "Abolish the Pauper Laws," *Social Service Review*, 8 (March 1934): 1–16.

¹¹⁵ Grace Abbott to Lillian Wald, February 25, 1937, Abbott MSS, Box 72, folder 7.

¹¹⁶ In this feeling, social insurance advocates were reflecting a sensibility widespread throughout the working class as well. Its presence does not imply that only men felt the "dole" as humiliating; there is ample evidence that poor women hated the infantilization and loss of privacy that came with casework. The gendering of that rejection was a secondary, not a primary, attribute—although this is often misunderstood. One is reminded of the once-widespread view that slavery was somehow more of an assault on masculine than feminine identity. The more intense male rejection of casework may have come from the inability of these middle and upper-class male reformers to adopt a parental stance toward the poor and the ease with which their female cohort adopted a maternal one.

¹¹⁷ Warner, Queen, and Harper, *American Charities and Social Work*, 560.

in 1922 of making social work “a profession, a ‘man’s job,’” he was speaking for many.¹¹⁸ Writing about 1890s social work, Queen and Ernest Bouldin Harper constructed sentences with the plural subject, “they”; in describing professional social work in 1928, their subject was always “he.”¹¹⁹ Yet in attempting to attain professionalism and defeminization of the field through casework, social workers were choosing an approach and a technique perceived by many as quintessentially feminine. Moreover, as a technique that required close contact with welfare recipients, casework served to strengthen commonalities between trained and untrained social workers and thus to retard the removal of amateurs from welfare agencies.

Equally important, women’s relative lack of interest in social insurance came from the fact that it was based on wage earning. Employers or employees might contribute, but the majority of women were members of neither of these groups. Those women who were employees were disproportionately ethnic or racial minorities, usually disregarded by the white women’s social reform network. Social insurance seemed different from public assistance primarily because it taxed the population at a different moment in the circulation of goods—the wage transaction—and thus it taxed primarily white men and gave primarily to white men.

The fiction that public insurance was different from public relief was in a mutually supportive relation with an older fiction, that women were economic dependents of male breadwinners and that women would therefore be taken care of by men’s insurance. This understanding of the sexual division of labor was known in this period as the family wage: men were to be breadwinners, women were to remain domestic.¹²⁰ In fact, recent working-class history has demonstrated definitively that the family wage was not a reality for the great majority of the American working class, whose men rarely earned enough to support their families singlehandedly. Yet as a norm for desirable gender relations and family organization, it was accepted not only by most men but also by most women, middle-class and working-class alike.

Almost all welfare activists, male and female, endorsed the family-wage principle and considered that women’s employment was a misfortune or a temporary occupation before marriage.¹²¹ But there was a variety of interpretations of this norm. From a gendered perspective, there was a world of difference between a conservative reading that considered it mandatory for a proper woman to remain domestic and what we might call a womanist desire to build social respect for domestic labor, especially mothering, and awareness of the particularly low wages and terrible working conditions of employed women.¹²² The

¹¹⁸ Stuart Alfred Queen, *Social Work in the Light of History* (Philadelphia, 1922), 88.

¹¹⁹ Warner, Queen, and Harper, *American Charities and Social Work*, chap. 2.

¹²⁰ Martha May, “Bread before Roses: American Workingmen, Labor Unions and the Family Wage,” in *Women, Work and Protest: A Century of U.S. Women’s Labor History*, Ruth Milkman, ed. (New York, 1985), 1–21.

¹²¹ Even Florence Kelley, at the radical edge of this group, shared these views; see her “Minimum-Wage Laws,” *Journal of Political Economy*, 20 (December 1912): 1003.

¹²² This norm may seem odd among a group of women, many of whom had removed themselves from this sexual division of labor by not marrying. But examined more closely, this expectation helps explain their personal choices. They did not think it wrong to ask women to choose between family

union men's demand for the family wage, to earn enough to support families, had a third meaning, one probably supported by many working-class women. The family-wage view of the social insurance advocates was, for the most part, an unconsciously conservative one, based on the unexamined assumption that women's full-time domesticity was desirable for all concerned.

Moreover, the female reformers' views changed as the employment of married women increased. That there was a time lag in their understanding of the breadth and permanence of this increase should not be surprising, particularly in a group of women all born in the nineteenth century. Sybil Lipschultz has shown how between two key Supreme Court briefs defending protective legislation—*Muller vs. Oregon* in 1908, and *Adkins vs. Children's Hospital* in 1923—the arguments of the social feminists changed considerably. The *Muller* brief privileged sacred motherhood and treated women's wage labor as an anomaly that must be prevented from interfering with the true female vocation. *Adkins* argued from women's weaker position in the labor market and the need for government to intervene in order to create gender equity. It "was not a matter of protecting unborn offspring, as it had been in *Muller*; it became a concern for the health and social participation of women themselves."¹²³ During the 1920s, some influential female welfare activists began to doubt the efficacy of attempts to maintain the family-wage system and to note its serious costs for women.¹²⁴ But when the representatives in government of this women's network—Grace Abbott, Katherine Lenroot, and Martha Eliot—wrote the Aid to Dependent Children section of the Social Security Act of 1935, they continued the charity and casework model, not only disregarding ideas on social insurance but also declining to develop an approach that recognized mothers' increasingly frequent employment.¹²⁵

Their program rested both on the family wage and on the sexual division of labor among welfare advocates. They assumed that male-designed social insurance programs would aid poor women through their husbands and that they need provide only for a few exceptions. The alternative—long-term public provision for needy mothers—they feared as a form of pauperization. They opposed policies that would establish female-headed families on a permanent basis, and they did not strive to undermine the hegemony of family-wage thinking.

And yet they knew the consequences of sending money into families only through male heads of families. They knew the assumption that resources would be shared fairly among family members was unfounded. Many among them had

and public-sphere activity; and they understood that their choice of public-sphere activity was often an artifact of their economic privilege—they could afford to live without husbands. Note that this perspective was by no means characteristic of all "single" women, that is, women not in heterosexual couples; it was characteristic of many of these "single" women because of their class position. It is also important to bear in mind that African Americans had distinctly different values on this point.

¹²³ Sybil Lipschultz, "Social Feminism and Legal Discourse: 1908–1923," *Yale Journal of Law and Feminism*, 2 (Fall 1989): 131–60, quotation on 137.

¹²⁴ An excellent example is in Sophonisba Breckinridge, "The Home Responsibilities of Women Workers and the 'Equal Wage,'" *Journal of Political Economy*, 31 (1928): 521–43.

¹²⁵ There was a left-wing alternative to the Social Security Act that would have guaranteed old-age and other insurance to housewives as well as employees, but there is no evidence that any of this network—with the exception of Mary Van Kleeck who was by this time associated with the Communist Party's initiatives—supported it.

worked for agencies dealing with family violence, desertion, and drunkenness, and they knew the fallacy of considering men the universal protectors of women and women the beneficiaries of male support. The male social insurance advocates were mostly uninterested in directing grants to women, something British feminists were demanding. Abraham Epstein argued, "Nor is it the mission of family allowances to usher in a new relationship between man and wife . . . [I see] no apparent reason why the alleviation of this particular form of insecurity should have to carry with it a general reformation of the world."¹²⁶ But the female welfare advocates did want to reform the world, the family in particular, and believed the state could do it. Nevertheless, they refrained from challenging the family-wage principle.

Female welfare activists, in short, designed programs that would benefit women directly when male wage-earners failed them, but they did not usually support programs that encouraged women's independence from men. Thus *both* tracks of the two-track welfare system were designed to maintain the family-wage system. (That they did not succeed is another matter.) The gender differences we have seen were part of a *shared* gender system that produced similar visions of the economic structure of the proper family.

These visions diverged more in their assumptions about the nature of poverty and how to help the poor. Social insurance advocates tended to assume that the damages of poverty would be cured by money and jobs. They differed among themselves regarding how best to provide what was missing—how generously, at whose expense, in what relation to private capital and the federal system. The social work welfare advocates had a more complex view. In part, it was a feminist one. They attempted the difficult and perhaps counter-historical task of defending the value of women's traditional domestic labor in a capitalist-industrial context. They also tried to integrate social and psychological themes with an economic theory of poverty. They believed that the injuries of class were experienced through problems like alcoholism, defeatism, and violence as well as through inadequate food and shelter, and they considered the social insurance definition of poverty partial, reductive, and naïve. They had been influenced by the experience of social workers combating drinking and domestic violence, particularly by their own clients. Despite the social distance between them and the objects of their concern, they identified with the women and children hurt by intra-family abuse perhaps even more than with those hurt by societal abuse. They did not, in this period, suppose that economic independence might be a precondition for the self-esteem and self-development they sought to give to poor women. Nor did they develop a system of casework suitable to conditions under which public assistance provided for millions rather than thousands of clients. Nevertheless, their vision at its best—articulated most vividly not in their words but, for example, in their settlement-house work—anticipated recent projects for

¹²⁶ Epstein, *Insecurity*, 638–39. A notable exception here was Paul Douglas, influenced by his feminist first wife, Dorothy Wolf Douglas. In the 1920s, he argued for family allowances to be paid directly to mothers, ensuring both their wisest use and providing them with a measure of freedom from the "tyranny" of male economic control. Paul Douglas, "Family Allowance System as a Protector of Children," *Annals of the American Academy of Political and Social Science*, 121 (September 1925): 16–24.

the empowerment of the poor, such as community organizing, far more than did social insurance schemes.

But it was the social insurance advocates who moved toward what T. H. Marshall called "social citizenship," the notion of public provision as an entitlement.¹²⁷ And it was these men who argued that only in this way could the stigma of the dole and the humiliation of dependence be removed.

THESE CONCLUSIONS ARE EXPLORATORY, NOT DEFINITIVE. As important as conclusions is the formulation of research questions. The institutional focus of much welfare history has sometimes had the disadvantage of passing over the assumptions and values that underlay those institutions. A gender analysis can help render such assumptions visible if it incorporates maleness as well as femaleness. Moreover, failing to incorporate women's activism into welfare history may produce accounts that explain sex discrimination in terms of an undifferentiated "sexism" or ahistorical "patriarchy," when in fact that inequality was constructed in specific, contingent historical events, from women's as well as men's activity.

While this article pleads for the value of gender analysis, it also tries to demonstrate the inaccuracy of dichotomizing men and women. In the very process of a binary comparison, I hope to have shown that male and female were hardly discrete and opposite categories. Welfare reformers, whether men or women, shared many values, and these shared values were often themselves part of the gender order, such as their joint support of the family-wage system. In an area like welfare history, ignoring the impact of gender obscures some of the most fundamental patterns.

¹²⁷ T. H. Marshall, "Citizenship and Social Class," in *Citizenship and Social Class and Other Essays*, T. H. Marshall, ed. (Cambridge, 1950).

Appendix

The women and men in this sample were leading national advocates for public welfare or government officials responsible for welfare programs who were also important advocates of welfare. Those who were only the employees of welfare programs or institutions are not included. I included only those whose *major* reform identification was with welfare; thus I excluded those whose primary association was, for example, with labor. Among men, I excluded elected officials.

I identified the women and men in different ways, in order to convey the most indicative identification succinctly. For the men, I cited occupation not by specific position but by general category. Many of the women did not have occupations in the sense of employment, and those that did were overwhelmingly social workers or social work administrators. Therefore a strictly occupational identification would have been unilluminating. Instead, for the women, I cited their major area of welfare activism or their specific organization if it was a major one, because these identifications show patterns and networks. To do this among the men would have been less informative, since they did not group themselves so much into a few organizations.

Because many were active in several areas or had a variety of occupations during their lifetimes, the identifications given here do not necessarily conform to some figures in the text, for instance, how many were social workers or academics.

White Female Welfare Reformers

<i>Name</i>		<i>Main reform</i>
Edith	Abbott	Social work, academic
Grace	Abbott	Children's Bureau
Jane	Addams	Settlement
Beulah Elizabeth	Amidon	Social work
Mary	Anderson	Women's Bureau
Barbara Nachtrieb	Armstrong	Social Security
Florence Arzelia	Armstrong	Social Security
Clara Mortenson	Beyer	Children's Bureau
Emily Newell	Blair	Democratic Party
Cornelia Foster	Bradford	Settlement
Sophonisba Preston	Breckinridge	Social work, academic
Josephine Chapin	Brown	Social work
Eveline Mabel	Burns	Social Security
Ida Maud	Cannon	Medical social work
Joanna	Colcord	Social work
Grace Longwood	Coyle	Social work, academic
Caroline Bartlett	Crane	Sanitation reform
Neva Ruth	Deardorff	Social work
Mary W. (Molly)	Dewson	Democratic Party
Emily Wayland	Dinwiddie	Housing reform
Helena Stuart	Dudley	Settlement
Loula Friend	Dunn	Social work
Crystal (Catherine)	Eastman	Industrial health
Hannah Bachman	Einstein	Mothers' pensions
Martha May	Eliot	Children's Bureau
Katherine Pollak	Ellickson	Social Security
Harriet Wiseman	Elliott	Democratic Party

<i>Name</i>		<i>Main reform</i>
Lavinia Margaret	Engle	Social Security
Elizabeth Glendower	Evans	Consumers' League
Minnie Ursala	Fuller	Child welfare
Josephine Clara	Goldmark	Consumers' League
Pauline Dorothea	Goldmark	Consumers' League
Jean Margaret	Gordon	Consumers' League
Helen	Hall	Settlement
(Amy) Gordon	Hamilton	Social work, academic
Alice	Hamilton	Industrial health
Jane Margueretta	Hoey	Social Security
Lucy Virginia Dorsey	Iams	Housing reform
Helen	Keller	Health reform
Florence Molthrop	Kelley	Consumers' League
Frances (Alice)	Kellor	Immigrant welfare
Julia Clifford	Lathrop	Children's Bureau
Katherine Frederica	Lenroot	Children's Bureau
Sophie Irene Simon	Loeb	Mothers' pensions
Emma Octavia	Lundberg	Children's Bureau
Amy	Maher	Social Security
Lucy Randolph	Mason	Consumers' League
Mary Eliza	McDowell	Settlement
Eleanor Laura	McMain	Settlement
Frieda Segelke	Miller	Women's Bureau
Belle Israels	Moskowitz	Democratic Party
Pauline	Newman	Women's Bureau
Frances	Perkins	Social Security
Agnes L.	Peterson	Women's Bureau
Mary Elizabeth	Pidgeon	Women's Bureau
Jeannette Pickering	Rankin	Congresswoman
Elizabeth Brandeis	Raushenbush	Unemployment
Agnes Gertrude	Regan	Social work
Mary Ellen	Richmond	Social work
Josephine Aspinall	Roche	Consumers' League
(Anna) Eleanor	Roosevelt	Social work
Rose	Schneiderman	Labor
Belle	Sherwin	Club
Mary Kingsbury	Simkhovitch	Settlement
Gertrude Hill	Springer	Social work
Mary Elizabeth	Switzer	Social work
(Julia) Jessie	Taft	Social work
M. Carey	Thomas	Education
Charlotte Helen	Towle	Social work, academic
Gertrude	Vaile	Social work
Mary Abby	Van Kleeck	Women's Bureau
Lillian D.	Wald	Settlement
Sue Shelton	White	Democratic Party
Edith Elmer	Wood	Housing reform
Helen Laura Sumner	Woodbury	Children's Bureau
Ellen Sullivan	Woodward	Social work

White Male Welfare Reformers

<i>Name</i>	<i>Occupation</i>
Felix	Minister
Will	Minister
Arthur	Welfare administrator
John	Welfare administrator
Frank	Welfare administrator
George Edmund	Economics professor
Jacob	Social work administrator
William Dwight	Minister
Clarence Monroe	Welfare administrator
Louis	Judge
John Graham	Minister
Frank	Social work, academic
Allen	Educator
Amos	Penologist
Ewan	Economist
Wilbur	Welfare administrator
John	Economics professor
Thomas	Welfare administrator
Michael Marks	Social work administrator
Miles Menander	Lawyer
Robert Weeks	Lawyer
Edward Thomas	Social work administrator
Carroll Warren	Economist
Richard Theodore	Economics professor
Abraham	Welfare administrator
Mordecai	Government economist
Isidore Sydney	Public health professor
Henry Walcott	Economics professor
Homer	Social work administrator
Lee Kaufman	Insurance executive
Felix	Judge
John Mark	Foundation executive
William	Journalist
Hastings Hornell	Minister
Charles Richmond	Sociology professor
William	Welfare administrator
Frederick Ludwig	Statistician
Harry	Welfare administrator
Robert	Social worker
Harold	Lawyer
Paul	Journalist
John Adams	Foundation executive
William Morris	Government economist
Samuel McCune	Sociology professor
Benjamin	Judge
Owen	Minister
Harry	Social work administrator
Valentine	Financier
Alexander Jeffrey	Minister
J. Prentice	Social work administrator
Louis Heaton	Lawyer
Paul A.	Social insurance administrator
James Bronson	Lawyer
Jacob August	Journalist
Raymond	Lawyer

<i>Name</i>		<i>Occupation</i>
Isaac	Rubinow	Physician
John Augustine	Ryan	Priest
Henry Rogers	Seager	Economist
Nathan	Sinai	Welfare academic
Charles	Stelzle	Minister
Isaac Newton Phelps	Stokes	Architect
James Graham Phelps	Stokes	Physician
Elwood Vickers	Street	Welfare administrator
Linton Bishop	Swift	Welfare administrator
Edgar	Sydenstricker	Statistician
Graham	Taylor	Minister
Rexford	Tugwell	Economics professor
Lawrence Turnure	Veiller	Welfare administrator
William	Walling	Nonemployed by choice
Adna Ferrin	Weber	Economics professor
Walter Mott	West	Social work administrator
Aubrey	Williams	Welfare administrator
Frederick Howard	Wines	Statistician
Edwin Edward	Witte	Economics professor
Robert Archey	Woods	Social work leader
Benjamin Emanuel	Youngdahl	Welfare administrator

Mass Culture, Mass Parliamentary Politics, and Modern Anti-Semitism: The Dreyfus Affair in Rural France

NANCY FITCH

BETWEEN 1986 AND 1988, A STATUE HONORING Captain Alfred Dreyfus gathered dust in a foundry, since no one wanted the memorial to the Jewish army officer unjustly accused of selling secrets to the Germans in 1894. The placement of the statue should have been a trivial issue. It proved otherwise. Although the French government had rehabilitated Dreyfus in 1906, when sculptor Louis Mitelberg tried to put his statue in the Ecole Militaire, which is where the army originally stripped Dreyfus of his rank, the minister of defense objected. The government suggested locating it in front of the Ecole Polytechnique, where Dreyfus received his military training. The school refused. Mitelberg proposed placing it opposite the Palais de Justice, where the French court of appeals overturned Dreyfus's court martial. The conservative government of Premier Jacques Chirac hesitated to move it there. Finally, almost immediately after socialist François Mitterand reinforced his power by defeating Chirac in the French presidential elections of 1988, Mitterand had the monument lifted onto a pedestal near a statue of Popular Front hero Léon Blum in the Garden of the Tuileries in Paris.¹

Few modern French figures have been more controversial than Dreyfus. Hundreds of books and articles have appeared on the Affair linked to his name since the army first convicted him in 1894. Dreyfus became a symbol of government witch hunts, of anti-Semitism, and—in his pardon—of the success of liberal humanism and democratic institutions at work. Until recently, few historians have been able to write about modern France without discussing the Affair.²

Yet, in spite of the controversy, which still seemed to plague France in the late 1980s, several historians have recently challenged the significance of the Affair as a turning point in modern French history and have questioned whether it split the nation as contemporaries had believed. They have argued that, on the contrary, a large majority of French, especially those who lived in rural France, remained

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¹ Stanley Meisler, "The Dreyfus Affair—It Never Dies," *Los Angeles Times*, October 30, 1986; and Stanley Meisler, "Paris Finally Finds a Place for Dreyfus Statue," *Los Angeles Times*, June 9, 1988.

² Michael Burns, "'Qui ça Dreyfus?': The Affair in Rural France," *Historical Reflections*, 5 (1978): 115.

indifferent to the Affair, that it was primarily a Parisian scandal.³ For these historians, many if not most French people regarded the Jewish question as “a minor question or no question at all.”⁴ According to these historians, peasants—who constituted approximately half the French population in 1896—were not taken in by the anti-Dreyfusard argument that all French must fight against the international Jewish conspiracy. Nor were they affected by the Dreyfusard argument that conscientious people everywhere must stand up and preserve French legal institutions. Uninterested in the larger issues revealed by the Affair, peasants simply found it a convenient source of new epithets and invectives they could adapt for their own uses, invoking the names of personalities from the Affair in their local *fêtes*.⁵

At its simplest, the argument that most of rural France remained oblivious to the Dreyfus Affair seems to be part of an effort to demonstrate that peasants remained in a world of their own, remotely connected to that of urban France.⁶ Although the number of literary and artistic references to the impact of the press in shaping the popular perception of Jews during the Dreyfus Affair is extraordinary, historians arguing for the isolation of rural France suggest that the influence of the mass distribution of printed material related to the Affair was minimal. In short, these historians seem to question whether one can speak of either mass politics or mass culture in *fin-de-siècle* France.⁷

The argument of peasant indifference to the Dreyfus Affair rests almost entirely on the conclusions of historian Michael Burns, who claims to have shown that “significant numbers of peasants remained indifferent to the Affair, not because they were unable to understand its complexities (urban folk suffered the same confusion), but because it failed to strike a relevant chord in villages and hamlets.”⁸ Ironically, Burns presents considerable evidence that news of the

³ The most recent restatement of this argument appeared in Paula Hyman, “The Dreyfus Affair: The Visual and the Historical,” *Journal of Modern History*, 61 (March 1989): 88–109. See also Michael Burns, *Rural Society and French Politics: Boulangism and the Dreyfus Affair, 1886–1900* (Princeton, N.J., 1984); Charles Sowerwine, “Review of *Rural Society and French Politics*,” *Journal of Modern History*, 58 (June 1986): 560–62; Eugen Weber, “Comment la Politique Vint aux Paysans: A Second Look at Peasant Politicization,” *AHR*, 87 (April 1982): 386; Eugen Weber, *France: Fin-de-Siècle* (Cambridge, Mass., 1986), 124–25, 267 n; Eugen Weber, “Foreword,” Norman L. Kleeblatt, ed., *The Dreyfus Affair: Art, Truth, and Justice* (Berkeley, Calif., 1987), xxviii; Edward Berenson, “Politics and the French Peasantry: The Debate Continues,” *Social History*, 12 (May 1987): 213–29; Paul Mazgaj, “The Origins of the French Radical Right: A Historiographical Essay,” *French Historical Studies*, 15 (Fall 1987): 292; Eugen Weber, “Reflections on the Jews in France,” in Frances Malino and Bernard Wasserstein, eds., *The Jews in Modern France* (Hanover, N.H., 1985), 8–27; Roger Magraw, *France 1815–1914: The Bourgeois Century* (London, 1983), 275. See also Jean-Marie Mayeur and Madeleine Rebérioux, *The Third Republic from Its Origins to the Great War, 1871–1914*, J. R. Foster, trans. (Cambridge, 1984), 197–98; Jean-Pierre Peter, “Dimensions de l’Affaire Dreyfus,” *Annales*, 16 (November–December 1961): 1141–67; Janine Ponty, “La presse quotidienne et l’Affaire Dreyfus en 1898–1899: Essai de typologie,” *Revue d’histoire moderne et contemporaine*, 21 (April–June 1974): 193–220.

⁴ Wasserstein, “Preface,” *Jews in Modern France*, x.

⁵ Burns, *Rural Society*, 143–63, 169–70; Mayeur and Rebérioux, *Third Republic*, 44–45, 55; Jean Lhomme, “La crise agricole,” *Revue économique*, 21 (1970): 550.

⁶ Burns, *Rural Society*, 103, 140–42, 160, 164, 168–69, 172; and Burns, “‘Qui ça Dreyfus?’”

⁷ Philip Dennis Cate, “The Paris Cry: Graphic Artists and the Dreyfus Affair”; and Susan Rubin Suleiman, “The Literary Significance of the Dreyfus Affair,” both in Kleeblatt, *Dreyfus Affair*, 62–95, 117–39.

⁸ Michael Burns, “Review of Stephen Wilson, *Ideology and Experience: Antisemitism in France at the Time of the Dreyfus Affair*,” *Social History*, 9 (May 1984): 258.

Affair reached the countryside and worked its way into local political disputes. Peasants read about it, talked about it, joked about it, fought about it.⁹ His argument is not that peasants lacked knowledge of the Affair but that the rural response to it differed sharply from that in French cities. In particular, he argues that rural "reactions unfolded in the context of distinct local rituals far from the view, far from the understanding, of government officials and other, largely urban, observers." When they were interested in the Affair, peasants "articulated their interest through an ancient, particularistic vocabulary."¹⁰

There were isolated examples of rural demonstrations against reopening the Dreyfus Affair, and *revanchiste* anti-Dreyfusard veterans' clubs existed in parts of the countryside.¹¹ However, there is no reason to challenge Burns's argument that rural politics remained less driven by anonymous crowd behavior than the new urban politics based on leagues and mass movements. Thus, although Burns acknowledges that anti-Semitic propaganda reached the peasantry, his interpretation reinforces an important revisionist argument. This argument views the emergence of an anti-Semitic, nationalist politics—the formation of a "radical right"—in *fin-de-siècle* France as an urban movement grounded in one tendency of extremist, if disenchanted, left-wing politics.¹²

While one cannot dismiss the significance of the "radical right" in many French cities, a great deal of evidence also points toward its influence, especially in the form of organized political anti-Semitism, in the countryside.¹³ The nationalist upheavals of the Dreyfus years had a political impact on rural France. Anti-Dreyfusard candidates with the help of the press deployed anti-Semitic themes in the legislative elections of 1898 in order to appeal to rural populations shaken by the economic downturn of the time. Finally, the political struggles of these years altered the nature of rural politics, primarily because anti-republican conservatives and conservative republicans used a metalanguage linking anti-Semitism to treason in order to succeed in the mass parliamentary politics of the late nineteenth century.

The effects of the *fin-de-siècle* political struggles in the countryside were evidenced in several important developments. In 1898, shortly after the worst rioting associated with the Dreyfus Affair, twenty-two anti-Semitic candidates were elected to the Chamber of Deputies.¹⁴ Only five of these represented urban constituencies. Four were elected in Algeria. Rural voters returned the rest.

⁹ See also the examples presented by Weber in "Comment la Politique Vint aux Paysans," 386–87.

¹⁰ Burns, *Rural Society*, quotation from 136.

¹¹ There was, for example, a "journée anti-sémite" in the village of Ronnet, in the Allier during January 1898. Someone in the same village began publishing an anti-Semitic newspaper in 1928. André Touret, "Les campagnes bourbonnaises sous la III^{ème} République de 1871 à 1914: Etude économique et sociale" (Mémoire de Maîtrise, Clermont-Ferrand, 1974), 195; Archives Départementales de l'Allier [hereafter, ADA] M.9.c (4), "Listes des associations dans le département, 1899."

¹² Paul Mazgaj, *The Action Française and Revolutionary Syndicalism* (Chapel Hill, N.C., 1979); Mazgaj, "Origins of the French Radical Right"; Philip Nord, *Paris Shopkeepers and the Politics of Resentment* (Princeton, N.J., 1986); Zeev Sternhell, *La droite révolutionnaire: Les origines françaises du fascisme, 1885–1914* (Paris, 1978).

¹³ Nord, *Paris Shopkeepers*.

¹⁴ These men identified themselves as members of the Anti-Semitic Party, although all of them explicitly rejected the idea that political parties could or should play a mediating role in politics. They saw themselves as and functioned more like an interest group.

Moreover, forty additional deputies who voted to disenfranchise the Jewish population of Algeria in February 1899 overwhelmingly represented rural areas and had themselves employed anti-Semitic or anti-Dreyfusard language in their campaigns of 1898. Collectively, they came from well over one-third of all French departments, largely from regions that Eugen Weber identified as among the most rural and often isolated parts of France: the West (the departments of the Seine-Inférieure, Côtes-du-Nord, Ille-et-Vilaine, Loire-Inférieure, Mayenne, Maine-et-Loire, and Orne), the Southwest (the departments of the Gers and Landes), the Southeast (the departments of the Var and Alpes-Maritimes), and the Massif Central (the department of the Corrèze).¹⁵ Anti-Semitic rhetoric also emerged in several additional elections held in rural areas of the central departments of the Allier, Cantal, Cher, Creuse, Haute-Vienne, Loire, and Lozère; of the western departments of the Charente, Charente-Inférieure, Deux-Sèvres, Gironde, and Morbihan; and in much of rural Mediterranean France, where the popular Vigné d'Octon ran for election as an "Aryan."¹⁶

If we take into account the constituencies that returned committed anti-Semites in addition to those that elected deputies who appealed to anti-Dreyfusard nationalism or anti-Semitism in their campaigns, it is fair to conclude that the Dreyfus Affair probably affected most rural departments in 1898. As *La croix nantaise* editorialized, "In a Chamber where the Jews reign, . . . the entrance of these nationalists and anti-Semites is a little revolution."¹⁷ This conclusion is not inconsistent with the argument that the impact of the Dreyfus Affair in the countryside depended on local concerns and was expressed in forms and

¹⁵ Stephen Wilson, *Ideology and Experience: Antisemitism in France at the Time of the Dreyfus Affair* (London, 1982), 213–29; Robert F. Byrnes, *Antisemitism in Modern France*, Vol. 1: *The Prologue to the Dreyfus Affair* (New Brunswick, N.J., 1950), 256–57; Jean Pascal, *Les députés bretons de 1789 à 1983* (Paris, 1983), 337–83; André Siegfried, *Tableau politique de la France de l'Ouest sous la Troisième République* (Paris, 1964), 78–96; and France, Chambre des Députés, 7th Législature, Session Extraordinaire de 1899, *Programmes professions de foi et engagements électoraux de 1898* (Paris, 1899), 165–66, 186–87, 277–83, 307–16, 379–81, 385–88, 416, 419–20, 469–72, 746–48, 754–55. This source is subsequently referred to as *Programmes, 1898*, although it is usually catalogued under the date 1899.

¹⁶ Anti-Semitic rhetoric emerged in the elections held in all four of the departments Michael Burns used as case studies. Four committed anti-Semites were elected in the Gers. Two additional deputies who supported legislation restricting Jewish citizenship were elected from the Orne and the Vendée. Octave Chenavaz, president of an agricultural syndicate in the Isère, argued in his successful campaign that the Dreyfus Affair demonstrated the weakness of the government and the power of Jewish high finance in France. Representing the largely urban district of Grenoble, Alexandre Zévaès, also of the Isère, was the only victorious candidate to come out strongly in favor of Dreyfus in 1898. Anti-Semitism surfaced in an additional election held in the Gers and in elections held in the Marne and in the Savoie, where M. Forni of Albertville argued that voters must save the country from the cosmopolitan menace threatening France in the Dreyfus Affair. Even considering the self-proclaimed anti-Semites, those who supported legislation restricting Jewish citizenship, and those who employed anti-Semitic or anti-Dreyfusard rhetoric in their successful electoral campaigns, the significance of the Dreyfus Affair and anti-Semitism in the 1898 elections is still understated, since none of this evidence includes information on those who lost elections. France, *Programmes, 1898*, 26, 101–04, 126–37, 149–50, 195–97, 330–39, 415, 435–36, 517–18, 614–15; Leo A. Loubère, *Radicalism in Mediterranean France: Its Rise and Decline, 1848–1914* (Albany, N.Y., 1974), 160–61; Patrice L.-R. Higonnet, *Pont-de-Montvert: Social Structure and Politics in a French Village, 1700–1914* (Cambridge, Mass., 1971), 129; Pierre Vallin, *Paysans rouges du Limousin: Mentalités et comportement politique à Compeigné et dans le nord de la Haute-Vienne (1870–1914)* (Paris, 1985), 227–28; Wilson, *Ideology and Experience*, 227–29; Burns, *Rural Society*, 14, 38–54.

¹⁷ *La croix nantaise*, June 5, 1898.

language that resonated with regional populations. But we must also explore how national ideas shaped local politics and how local politics informed and influenced the development of powerful ideologies. Even in Paris, as Philip Nord has shown, the impact of the Dreyfus Affair depended on the intersection between larger, national ideologies and the local concerns of the lower middle and working classes.¹⁸

The Dreyfus Affair became an affair and not just an unhappy case of miscarried justice because it generated arguments and ideas that transcended the outbursts of anti-Semites. The fact that these arguments and ideas could be expressed in a myriad of particularistic vocabularies with numerous local nuances is precisely why the Affair could become the source of a powerful new nationalism with ramifications beyond France and into the future. Too often, historians have explained the emergence of new ideologies by analyzing the writings of the major figures who espoused them. These analyses tell us much about the ideologies but very little about how they worked. Only local studies can demonstrate where and why new ideologies were taken seriously or ignored.

The nationalist, anti-republican, and anti-Semitic ideas associated with the Affair were “received, rejected, or acted upon” in particularly significant ways in rural areas of the departments of the Gers and the Allier.¹⁹ These departments represent extremes of developments echoed throughout much of the country. In no single department did anti-Semitism succeed as an electoral discourse better than in the Gers, where five out of five victorious candidates either resorted to anti-Semitic slogans or arguments in their campaigns or joined the group of committed anti-Semites in the Chamber of Deputies after the elections. In contrast, although the language of anti-Semitism and references to the Dreyfus Affair shaped much of the rhetoric of the 1898 elections in the Allier, they fell on so many deaf ears that anyone who examines the elections from the perspective of national success could easily miss the impact of the Affair in the department. In both departments, the perceptions of many voters were influenced by the impact of the low-cost and small-format press.

BY THE 1880s, CHEAP, MASS-PRODUCED NEWSPAPERS began to circulate throughout France. The development of the Linotype, the rotary press, and photogravure all made the new, inexpensive, mass-circulated newspaper possible. In 1858, the circulation of Parisian dailies collectively amounted to no more than 235,000 copies. By 1870, the circulation had risen to over one million. That figure doubled by 1880 and rose to five million by 1910. By this time, too, a lively provincial press had developed.²⁰

Not only did newspapers begin to circulate on an unprecedented scale, they appeared in new forms as well. Even local newspapers produced in the 1870s had

¹⁸ Nord, *Paris Shopkeepers*.

¹⁹ Burns, *Rural Society*, 31.

²⁰ Theodore Zelden, *France 1848–1945: Taste and Corruption* (London, 1980), 192; Phillip Dennis Cate and Sinclair Hamilton Hitchings, *The Color Revolution: Color Lithography in France, 1890–1900* (New Brunswick, N.J., 1978).

little to say to ordinary people. Journalists based their arguments on historical references only the most educated could understand. The newspapers that became most popular after 1880, in particular *Le petit journal*, *Le petit parisien*, and the Catholic Assumptionist *La croix*, used accessible language and pioneered the use of illustration, especially the political cartoon. They were entertaining, combining fiction in the form of *roman-feuilletons* or serialized novels and sensationalist “true” stories (*faits divers*) based on extraordinary crimes and scandals, ideally involving well-known, “public” persons. They were also cheap, costing no more than a few *sous*.²¹

Not surprisingly, newspapers quickly acquired an influence in rural areas. Thanks to the public school reforms of the 1880s, rural illiteracy was all but eliminated even in the central and southwestern departments where it had previously been widespread.²² Peasants in many departments, including those in the Gers and the Allier, were reading a wide variety of newspapers by 1898. In the Gers, several national, regional, and local newspapers distributed as many as 10,000 copies throughout the department, which boasted a population of no more than 230,000 people.²³ In the Allier, too, newspapers circulated in rural areas. A government survey completed in 1893 identified newspaper sellers in even the smallest rural villages. Here, readers preferred republican papers and the little weekly editions of the Parisian press. Priests gave away copies of *La croix* at Mass. In isolated hamlets, children distributed copies of *Le petit journal*, which serialized *La France juive*, a vitriolic book by France’s most prominent anti-Semite, Edouard Drumont. Drumont’s newspaper, *La libre parole*, sold well in the rural village of Saligny. While visiting the Allier, Dreyfus supporter Daniel Halévy met a peasant who possessed a copy of Drumont, several newspapers, and a Bible bought from a peddler. Several peasants on different occasions indicated that they enjoyed elections because candidates would give away or sell inexpensive newspapers.²⁴ Moreover, cultivators from several rural villages wrote letters to socialist weeklies that circulated in the countryside.²⁵ By 1910, *Le petit parisien* marketed 75 percent of its papers in the provinces. During 1911, *Le petit journal* sold about 80 percent of its 800,000 daily issues outside of Paris.²⁶

Landlords in the Allier tried to prevent sharecroppers from reading anything

²¹ Zeldin, *France 1848–1945*, 178–225; Jacques Lelong, “L’information dans la presse locale: Il y a un siècle (2–15 mai 1877),” *Notre Bourbonnais* (2^{ème} trimestre 1977): 427–33; Eugen Weber, *Peasants into Frenchmen: The Modernization of Rural France, 1870–1914* (Stanford, Calif., 1976), 452–70; Robert L. Hoffman, *More Than a Trial: The Struggle over Captain Dreyfus* (New York, 1980), 101–04; 145.

²² Weber, *Peasants into Frenchmen*, 468–70; François Furet and Jacques Ozouf, *Reading and Writing: Literacy in France from Calvin to Jules Ferry* (Cambridge, 1982).

²³ Maurice Bordes, “L’évolution politique de Gers sous la III^{ème} République,” *L’information historique*, 23 (1961): 22; Bernadette Suau, “La Gascogne landaise,” in Maurice Bordes, et al., *Histoire de la Gascogne contemporaine des Landes aux Pyrénées* (Roanne, 1983), 59–61.

²⁴ *L’indépendant*, September 19, 1889; ADA T.912, “Listes des vendeurs de journaux dans l’Allier, 1893”; ADA 4M non-classé, “Le sous-préfet de Gannat au préfet, 25 juin 1879”; Archives Nationales [hereafter, AN] F⁷ 12460, “Anti-Sémitisme dans les départements”; Daniel Halévy, *Visites aux paysans du Centre* (Paris, 1934), 130–31; and *La socialiste de l’Allier*, May 1, 1898, May 6, 1898.

²⁵ *La socialiste de l’Allier*, 1898–1900.

²⁶ Byrnes argues that each sold a million papers in the provinces in the 1890s, but his figures cannot be correct. Data on newspaper circulation for 1898 is difficult to obtain. One only knows, for example, that *La croix du Nord* boasted of “an enormous circulation.” Byrnes, *Prologue to the Dreyfus Affair*, 286–90; Ponty, “La presse quotidienne,” 193–220.

other than *La croix*, a journal that, before becoming virulently anti-Semitic, promoted traditional values associated with the nobility and the clergy. Occasionally, landlords also gave away small, local propaganda sheets, but too many additional sources of information existed for them to control sharecroppers' attitudes. Thus when one sharecropper asked another how he had acquired his republican ideas in the censured environment of the rural Allier, the other responded, "My dear man, I have acquired the habit of reading my newspaper [*La croix*] topsy-turvy."²⁷

Newspapers are significant in an exploration of rural anti-Semitism because, from its earliest stages, the case against Dreyfus was tried in the press, especially in *La croix*, which was so widely dispersed it could not be ignored.²⁸ In another time, the treason of a high army officer might have been treated as an internal army affair. But, even though the evidence against Dreyfus was nearly nonexistent and much of what the newspapers printed consisted of no more than rumors, over time the rumors published in newspapers acquired the force of truth. At the end of 1896, it seemed that almost no one outside of Dreyfus's immediate family believed he could be innocent. It was commonly understood that the entire department of the Landes "accepted Dreyfus's guilt." Even sharecropper Emile Guillaumin, who later argued against the anti-Semitic right, wrote to a friend in late 1897 that "Dreyfus is probably guilty, but if he is innocent, it is truly a major tragedy."²⁹

After exhausting appeals to the army and several political representatives including the president of the Republic, the Dreyfus family recognized they could get no support for Dreyfus without access to the press. With this in mind, they took their evidence to the popular novelist, Emile Zola, who outraged the government in January 1898 by publicly charging the army's highest officers with concealing evidence in an open letter that appeared on *L'aurore's* front page. He described his public intervention in politics in the newspaper by pointing out that "what I do is only a revolutionary method of hastening on the explosion of truth and justice." Indicative of his success, this letter merely represented the first of Zola's public political interventions on the front page of *L'aurore*.³⁰

Zola's letter set off anti-Dreyfusard disturbances and an extraordinary cultural production designed to satiate ordinary people's desire for news about the Affair. Newspapers probably constituted the most important commodity marketed, as they began in these years to replace the Chamber of Deputies as the chief site and instrument of public debate. Papers representing different political tendencies

²⁷ Emile Guillaumin, *En Bourbonnais* (Paris, 1902), 26, 26 n. Compare Burns, *Rural Society*, 134, where he states that *La croix* was "the only available paper" in the Allier.

²⁸ Burns is especially effective in discussing the role of *La croix* in disseminating anti-Semitic propaganda in the countryside. Burns, *Rural Society*, 131–37. See also Pierre Sorlin, "*La croix*" et les juifs (Paris, 1967), 42–44.

²⁹ Emile Guillaumin to Gillardin, November 28, 1897, ADA 47 J 120, "Fonds Guillaumin." See also the exchange between Georges Valois and Emile Guillaumin in ADA 47 J 90, "Fonds Guillaumin"; and Suau, "La Gascogne landaise," 56–57.

³⁰ Emile Zola, *Lettre à la France* (Paris, 1898), in Lee M. Friedman Collection, Houghton Library, Harvard University, *pFc9.D8262.zzx, "Miscellaneous, Affaire Dreyfus"; D. W. Brogan, *The Development of Modern France, 1870–1939*, Vol. I: *From the Fall of the Empire to the Dreyfus Affair* (New York, 1966), 335–36; Jean-Louis Bredin, *L'affaire* (Paris, 1983), 476.

debated each other in front-page sections called *Echoes*.³¹ Men and women in the countryside came to know life outside their local area through summaries of Parisian newspaper accounts. Newspapers allowed provincial men and women to become part of the crucial debates of the nation. Local commentary and events were relegated to back pages and fine print, if they appeared at all. No matter where one lived, politics took place in Paris.³² Consequently, politics in the countryside could no longer be entirely driven by local issues.

While mass-circulation newspapers alone did much to make the Affair what it was, their presence was made all the more relevant by an explosion in the production of anti-Semitic images, the most successful of which were printed in color. The use of visual images probably increased the circulation of small, sensationalist newspapers. *La croix* began publishing anti-Semitic cartoons in almost every paper it sold.³³

Although there can be no question that the production and consumption of cultural artifacts carrying political messages dates from the French Revolution or earlier, the new technology of the color print revolution combined with the growing mass market for artifacts differentiated the cultural production of the Dreyfus Affair from that of any previous event or election.³⁴ Most important, before the development of the new print technologies, it was difficult to produce the quantity of the cheap cultural commodities sold at the end of the nineteenth century. Most previous artifacts were in the form of images, songs, or almanacs specifically designed to promote a person or idea through identification with someone or something already known. As Burns has shown, "when a picture recalls old, familiar compositions, a deeper meaning is grafted to the subject."³⁵ For example, in the 1848 presidential election, Louis Napoleon combined his

³¹ See Lelong, "L'information dans la presse locale"; Jacques Lelong, "La presse bourbonnaise et les lois laïques de 1880 à 1882," *Notre Bourbonnais*, 9^{ème} série, no. 218 (4^{ème} trimestre, 1981): 421–34; *La croix de l'Allier*, January 16, 1898; Bredin, *L'affaire*, 476; Weber, *France: Fin-de-siècle*, 240; Wilson, *Ideology and Experience*, 9–11; and Byrnes, *Prologue to the Dreyfus Affair*, 286–90.

³² Burns points out that most Gers newspapers emphasized Parisian news; *Rural Society*, 135. See also Lelong, "La presse bourbonnaise"; Weber, *France: Fin-de-siècle*, 240; Zelden, *France 1848–1945*, 144–225; and Richard Terdiman, *Discourse/Counter-Discourse: The Theory and Practice of Symbolic Resistance in Nineteenth-Century France* (Ithaca, N.Y., 1985), 117–46.

³³ The analysis of *La croix* comes from my own reading of the newspapers from 1897 to 1900. See also Sorlin, *La croix*. Burns suggests that the illustrations may have been used to attract semi-literate peasants like those in the Gers, since he found more illustrations in *La croix du Gers* than he did in the other regional editions of *La croix* he examined. (See Burns, *Rural Society*, 135, 208 n.) But, curiously, the daily Parisian edition of *La croix* makes extensive use of illustrations, while *La croix de l'Allier* employed written text exclusively, at least before 1900. Since considerable evidence suggests that editors addressed *La croix de l'Allier* to rural readers and that the peasants of the Gers were more likely to be literate than their Allier counterparts, the relationship between the use of illustrations and their intended audience may be more complicated.

³⁴ References to and descriptions of the cultural production related to the Dreyfus Affair can be found in collections housed in the Archives Nationales, the Archives of the Prefecture of Police, Paris [hereafter, APPP], and departmental archives. Unfortunately, neither the police nor the prefectures routinely kept cultural artifacts with their notes about them. Hence, to find examples of the artifacts themselves, one must also consult special collections devoted to them. Many of the Dreyfus Affair artifacts discussed here can be found in the Lee M. Friedman Collection, Houghton Library, Harvard University. The collection is classified by type of artifact and in some cases by title. Other examples of these artifacts can be found in collections housed in the Musée Carnavalet and the Bibliothèque Nationale in Paris.

³⁵ Burns, *Rural Society*, 69.

image (and, by implication, his politics) in print, poetry, and almanacs with that of his famous uncle. More significant, the Democratic-Socialists pioneered the creation of national electoral clubs and organizations and also used what Burns has called “associative composition” in almanacs, catechisms, and songs in 1849 to assimilate their fairly modern political ideology into traditional popular culture.³⁶ Finally, General Georges Boulanger combined the strategies of both Louis Napoleon and the Democratic-Socialists to deploy pictorial techniques that linked him to the heritage of the two Napoleons through local political distribution networks.³⁷

The cultural artifacts produced during the Dreyfus years continued to employ “associative composition,” this time to reconfigure the traditional stereotype of the “usurious Jew” into a powerful symbol that could represent any and all social dangers. They also cited known historical events, again placing a new ideology—in this case, one based on the Jew as a signifier of everything that threatened France—into a familiar tradition of popular culture.

However, in contrast to previous, politically oriented cultural production, the cultural artifacts sold in the wake of the Dreyfus Affair emerged from a relatively autonomous environment. Organizations, popular presses, and manufacturers produced anti-Semitic artifacts and newspapers because they sold well. They produced large quantities for mass consumption in a national market. It is not clear that they sought to sell an idea; more likely, they believed they could sell commodities because many found anti-Semitic and anti-Dreyfusard caricatures to be entertaining.³⁸

Anti-Semitism was used to sell everything.³⁹ Stereotypes of Jewish usurers were sold as cartoons. Children ate chocolate wrapped in prints of popular Catholic and anti-Semitic figures and read illustrated books about the major issues of the Affair. They played with toys that allowed them to hang Dreyfus by the neck and drop Zola’s pants. (See Figure 1.) Men rolled cigarettes in papers printed with

³⁶ Edward Berenson, *Populist Religion and Left-Wing Politics in France, 1830–1852* (Princeton, N.J., 1984); Burns, *Rural Society*, 69.

³⁷ On Boulangist images, see Burns, *Rural Society*, 60–80; Francis Bergeron and Philippe Vilgier, *Les droites dans la rue: Nationaux et nationalistes sous la Troisième République* (Grez-en-Bouère, 1985), 36; Georges Rougeron, “Les débuts du mouvement ouvrier en Allier,” *Notre Bourbonnais*, 9^{ème} série, no. 212 (2^{ème} trimestre, 1980): 305; ADA T.345, “Imprimerie et librairie—Colportage.” On Napoleon, see André Rossal, *Napoleon: 50 documents facsimiles* (Paris, 1969); and Armand Dayot, *Napoleon: Raconte par l’image d’après les sculpteurs, les graveurs et les peintres* (Paris, 1895).

³⁸ All theories of popular and mass culture are controversial. Considerable research has demonstrated that the Dreyfus Affair was intimately related to the desire to sell newspapers. Similar and relatively uncontested arguments have been made vis-à-vis the emergence of “yellow journalism” in the United States and the outbreak of the Spanish-American War, an event that also took place in 1898. See Peter, “Dimensions de l’affaire Dreyfus”; Byrnes, *Prologue to the Dreyfus Affair*, 286–90; Marcus M. Wilkerson, *Public Opinion and the Spanish-American War* (Baton Rouge, La., 1932); and Joseph E. Wisan, *The Cuban Crisis as Reflected in the New York Press* (New York, 1934).

³⁹ Dreyfusards responded in kind, not because they thought Dreyfusard products would sell well but because they found it imperative to resist commercially the commercial products of the anti-Dreyfusards. The press was overwhelmingly anti-Dreyfusard before September 1898. Weber, “Foreword” to Kleeblatt, *Dreyfus Affair*; Weber, *France: Fin-de-siècle*, 240; Norman L. Kleeblatt, “The Dreyfus Affair,” in Kleeblatt, 1–24, esp. 2–12; Mayeur and Reberieux, *Third Republic*, 197, 200–01; Ponty, “La presse quotidienne,” 198–99, 201. On the technological change that made the Affair possible, see Walter Benjamin, “The Work of Art in the Age of Photo-Mechanical Reproduction,” in *Illuminations*, Harry Zohn, trans. (New York, 1969), 217–51; Cate and Hitchings, *Color Revolution*.



FIGURE 1: "The Last Judgment" was one of the most vicious paper novelties produced around 1898. One can pull on the man's arm to hang Dreyfus, the traitor. Dreyfus is labeled as a "dirty beast." The caption exclaims, "Extricate yourself here from [the beast's] infected paws!!!" It was one of many such artifacts produced during the Dreyfus Affair. Courtesy of the Musée Carnavalet, Paris.

visual stories of the Affair. One set of cigarette papers was called *les bordereaux*, after the *bordereau* or document that implicated Dreyfus. While men smoked up reproductions of the evidence used to convict Dreyfus, women cooled off with

fans illustrated with reprints of anti-Dreyfusard newspaper cartoons. People played with cards bearing the portraits of Dreyfus and Drumont and sent news to others on Dreyfusard and anti-Dreyfusard postcards.⁴⁰ The postcards took two forms. One consisted of photographs of significant events and people of the Affair, usually produced by the Dreyfusards. The other reproduced frames from anti-Dreyfusard posters as sets of cards that were collected. Illustrated broadsheets, songsheets, and board games recounted the Affair in great detail. "Wheel-toys," first introduced during the Boulanger period, circulated again during the Dreyfus Affair and provided multiple, popular interpretations of the Affair. Men, women, and children at local fairs in southern France purchased kaleidoscopes illustrating figures from the Dreyfus Affair for 2 *sous*.⁴¹

Anti-Dreyfusard graphic artists drew from a repertoire of cartoon images of Jews, most of which were familiar and demeaning. Many came from newspaper reproductions and derived their impact and authenticity from their newspaper associations. The imagery used in these artifacts was secular; even *La croix* used very little traditional Christian anti-Jewish caricature.

This does not mean, however, that anyone read these images literally. One of many examples of the impact of this cultural production can be seen in the action of a voter from the Allier who in 1898 cast as a ballot his own drawing mimicking a newspaper cartoon. France, embodied as Marianne in a Phrygian cap, crowns the controversial figure of Commandant Marie Charles Esterhazy, dressed in German military attire. An upside-down copy of "The Declaration of the Rights of Man" is pierced by a sword shaped like a cross, while blocked out in the large E of ANARCHIE are the names of bureaucrats usually identified with republicanism. The claim is that if France succeeds in exonerating Esterhazy, "anarchy will end up triumphant," ordinary republicans will be marginalized, and the Rights of Man will be destroyed. Marianne, *la gueuse* or the "slut" whom the "new right" identified with the left, has been transformed into a whore, whom the left can identify with the "new right." The ballot was evidently drawn by someone who understood well what was going on in the country and knew history. Yet it was probably produced by the same kind of republican peasant who read his newspaper "topsy-turvy."⁴² (See Figure 2.)

The use of mechanical reproduction in newspapers and artifacts dramatically changed the complexion of rural politics. On the one hand, large landowners could not isolate peasants, sharecroppers, and day laborers from Parisian and provincial cultural production. It circulated everywhere. As a character in one of

⁴⁰ Revolutionary cards and games that were nearly identical to those that appeared during the Dreyfus Affair were displayed at an exposition on "A Revolução Francesa e o Brasil" held in the Biblioteca Pública do Estado do Rio de Janeiro in July 1989. But the individuals portrayed did not have to be labeled on these artifacts.

⁴¹ Postcards that sold for 2 francs at the time they were produced in 1898 and 1899 sold for 60 or 70 francs in 1903. On postcards, see Xavier Granoux, *"L'affaire Dreyfus": Catalogue descriptif des cartes postales illustrées françaises et étrangères parues depuis 1894* (Paris, 1903), 1–5. See also Philippe Joutard, *La légende des camisards* (Paris, 1976), 314; and John Grand-Carteret, *L'affaire Dreyfus et l'image* (Paris, 1898), 35–58.

⁴² The drawing is from AN C.5104, "Elections Legislatives, 1898." On Marianne as *la gueuse*, see Karen Offen, "The Political Career of Paul de Cassagnac" (Ph.D. dissertation, Stanford University, 1971), 106; published in the Garland series as *Paul de Cassagnac and the Authoritarian Tradition in Nineteenth-Century France* (New York, 1991).

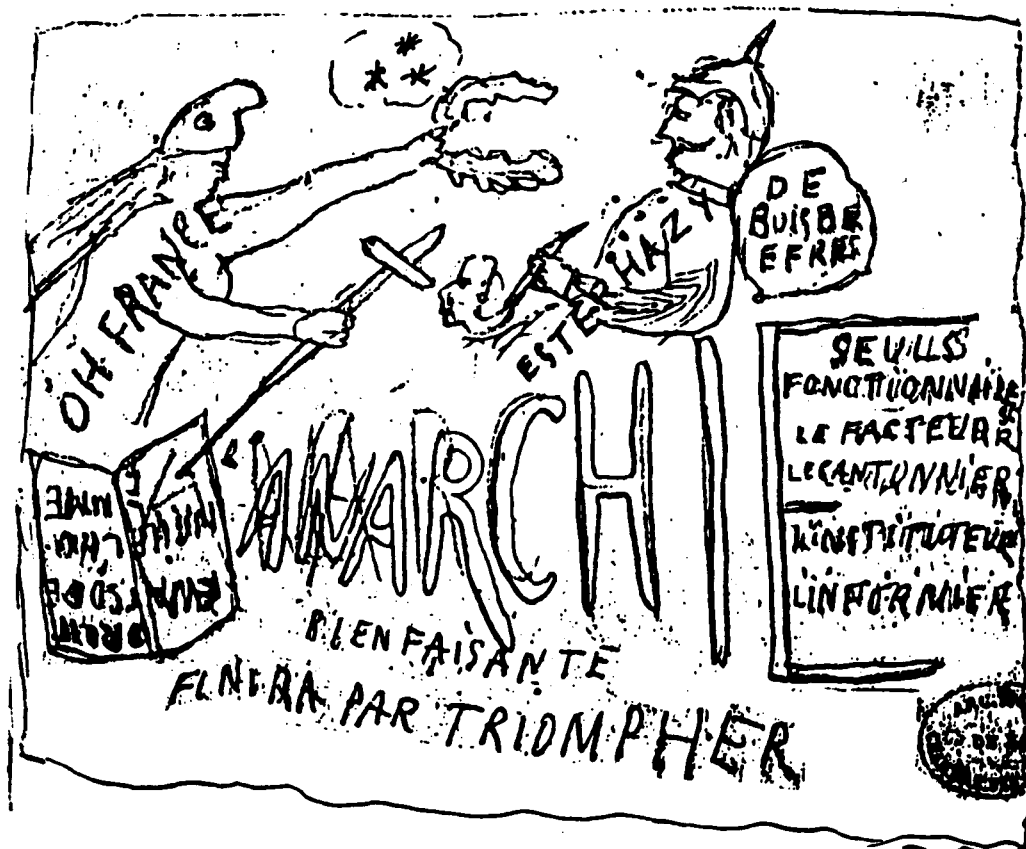


FIGURE 2: "Bountiful Anarchy will end up triumphant!" This figure was cast as a ballot by an Allier voter in the legislative elections of 1898. It was drawn in purple and orange crayon. Archives Nationales, Paris.

sharecropper Emile Guillaumin's novels said upon spying some colored posters in his Allier village, "Look! . . . those who can read have something to amuse them."⁴³ On the other hand, local notables did not always want to isolate peasants. Some, like Paul de Cassagnac, who published *Appel au peuple*, *L'électeur du Gers*, and *La voix du peuple* in the Gers, found the press as a site of politics consistent with their anti-parliamentarian positions. Others, like the *Progressiste* Camille Gabiat of the Haute-Vienne, found posters very useful. Gabiat profited from a poster that appeared throughout the Haute-Vienne on the eve of the 1898 elections. It accused his opponent, the Opportunist (republican) Henri Vacherie, of taking money from Jews to foment war and revolution. The previously popular Vacherie lost the election to Gabiat, and it may be, as one historian has suggested, that the posters and the anti-Semitic campaign associated with them undermined years of grass-roots organizing.⁴⁴ *La croix du Gers* reported that crowds loved the poster "La patrie en danger," which appeared in Auch (Gers) and featured anti-Semitic

⁴³ Emile Guillaumin, *The Life of a Simple Man*, Eugen Weber, ed. (Hanover, N.H., 1983), 186. See also AN F⁷ 12463, "Le Commissaire spécial de l'Allier au Ministre de l'Intérieur," November 9, 1898; ADA T.338 "Colportage des journaux, 1835-1887"; ADA T.345, "Colportage"; ADA T.334, "Colportage/Police de la Press"; Weber, *Peasants into Frenchmen*, 464 n.

⁴⁴ Offen, "Paul de Cassagnac"; Vallin, *Paysans rouges du Limousin*, 195-98.

slogans to combat “juiverie” (the Jewish problem). It was so effective that the newspaper wanted to place it throughout the district of Mirande (Gers) on the eve of the 1898 elections.⁴⁵ Newspapers, posters, and other cultural artifacts thus helped to shape ideas and made it difficult for rural dwellers to believe that parliamentary politics were merely a question of delivering on local patronage.

Without these materials, the Dreyfus Affair would never have come into the countryside. Very few peasants had any direct contact with Jews, for of the estimated 71,000 Jews in the country in 1897, 45,000 lived in Paris.⁴⁶ Most of the rest lived in other French cities. Indeed, it was perhaps because of their lack of direct contact that those rural folk unfamiliar with Jews may have been far more willing than their urban counterparts to accept the most egregious and diabolic stereotypes that appeared in the products of mass culture.

Although anti-Semites were not the first politicians to draw on images from popular culture to structure new ideologies, they were among the first to take advantage of new mass marketing techniques that popularized these images. Those who evoked them spoke in a language already known, drawing on tendencies that already existed.⁴⁷ The success of the images, however, lay in the anti-Semites' ability to reconfigure some significant traditional associations. In the process, they kept those associations alive in popular memories but in new forms.

TO UNDERSTAND HOW THE DREYFUS AFFAIR entered the countryside, we need to know something about rural life in the *fin-de-siècle* departments where it became important. Although local peculiarities tend to undermine all generalizations, many rural French perceived that they were experiencing a crisis. In the 1880s and 1890s, both peasants and large landowners—especially those associated with viticulture—faced acute economic difficulties as a consequence of the late nineteenth-century depression. Some of the more dramatic economic changes affected the regions of France that produced anti-Semitic deputies in 1898.⁴⁸ For example, in the Gers, an isolated rural department in the southwest corner of France, phylloxera and black rot had wiped out nearly three-quarters of the department's vines by 1898, especially in Armagnac. Peasants without insurance or access to low-interest loans lost their farms. The demand for industrial products, especially those used in the commercial production of Armagnac, an *eau-de-vie* produced from Gers grapes, declined dramatically with the reduction in acreage under the vine. Emigration, the only way out for many, created labor shortages for those who did survive the crisis.⁴⁹

⁴⁵ *La croix du Gers*, March 13, 1898.

⁴⁶ Mayeur and Rebérioux, *Third Republic*, 200.

⁴⁷ Wilson, *Ideology and Experience*, 606.

⁴⁸ Herman Lebovics, *The Alliance of Iron and Wheat in the Third French Republic, 1860–1914: Origins of the New Conservatism* (Baton Rouge, La., 1988), 26–50; *La socialiste de l'Allier*, January 3, 1897, February 20, 1898; Bordes, “L'évolution politique,” 19–22; Roger Brunet, *Les campagnes toulousaines: Etude géographique* (Toulouse, 1965), 367–400; Simone Derruau-Boniol, “Le socialisme dans l'Allier de 1848 à 1914,” *Cahiers d'histoire*, 2 (1957): 115–61; René Secretin, “Le Boulangisme dans l'Allier” (Mémoire pour la maîtrise et lettres, Clermont-Ferrand, October 1969), 12–31; Georges Courtes, *et al.*, “La Gascogne orientale,” in Bordes, *Histoire de la Gascogne*, 210–14.

⁴⁹ Burns suggests that the agricultural crisis was severe in the 1880s but that conditions improved

Given the labor shortage, large landowners also found themselves in a crisis. Having made small fortunes selling grain and calves during Louis Napoleon's Second Empire, many were helpless when American and Argentinian farmers and cattle ranchers took advantage of refrigeration and favorable French trade agreements to dump products on European markets. Many landowners tried to sell out as fast as they could, and large properties began to disappear in many parts of the Gers. Remarkably similar conditions characterized the departments of the Allier, the Gironde, the Landes, and the Loire-Inférieure.⁵⁰

Aggravated by the politics of the *Ralliement*, the movement launched when the pope encouraged all French Catholics to embrace the Republic, political failures accompanied many landowners' economic failures. Large landowners had tended toward conservative politics based on local social networks. Although many professed sympathy for monarchist and Bonapartist causes, they generally had avoided forming mass-based political parties with national programs and affiliations. Rather, they depended on prestige and patronage for rural votes.⁵¹ In the Gers, for example, local notables were almost unchallenged after the collapse of the Second Republic until they lost their prestige along with their profits and

during the 1890s. Using the same evidence he presents for the Gers, I am painting a much bleaker picture of the 1890s. He is correct in pointing out that at the end of the nineteenth century only 48,000 hectares in vines remained of the 108,000 cultivated in 1873. What he does not indicate is that the land in vines increased from 108,000 hectares to 122,000 hectares between 1873 and 1882. The land in vines then declined by 22 percent from 1882–1893 and by 49 percent from 1893–1902. These figures suggest that the crisis may have been even worse in the 1890s than in the 1880s. Wine production continued to decline in the 1890s, albeit not as rapidly as it had in the previous decade. The same can be said of property values. Prices for cereals and animals also continued to decline at the end of the nineteenth century, though perhaps not as rapidly as they declined earlier. Using data that Burns did not examine, I found that the price of wine in France actually increased in the 1880s, then fell fairly dramatically in the 1890s. Figures provided by historian Gilbert Garrier for the Beaujolais suggest that the purchasing power of the average vinegrower peaked in the 1880s, then fell precipitously in the 1890s, reaching a low point in 1898. Leo Loubère's data also demonstrates the significantly increased cost of producing grapes at the end of the century. Nationally, there was some temporary improvement in cereal prices in 1898 as a consequence of the Spanish-American War, but, in general, agricultural prices continued to fall until after 1900. Thus none of the data that either Burns or I examined suggests that "though not yet 'extremely favorable,' the situation in the rural Gers [on the eve of the Dreyfus Affair] was, beyond doubt, an improvement over the black decade of the 1880s"; see Burns, *Rural Society*, 38–42; Brunet, *Les campagnes toulousaines*, 382–83, 396–400; *La croix*, June 8, 1898; Bordes, *Histoire de la Gascogne des origines à nos jours* (Roanne, 1977), 366–68; Courtes, "La Gascogne orientale," 210–14; Leo Loubère, *The Red and the White: A History of Wine in France and Italy in the Nineteenth Century* (Albany, N.Y., 1978), 299; Gilbert Garrier, *Paysans du Beaujolais et du Lyonnais, 1800–1970*, Vol. II (Grenoble, 1973), 111–12, 117–22; Magraw, *France, 1815–1914*, 321–26; Lhomme, "La crise agricole," 521–53.

⁵⁰ Brunet, *Campagnes toulousaines*, 393; Bordes, *Histoire de la Gascogne*, 203–10, 366–68; Courtes, "La Gascogne orientale," 210–14. On the Allier, see ADA M.36.c, "Situation des ouvriers de l'Allier, 1884"; and *Bulletin de la Société de l'Agriculture de l'Allier*, from 1876, when it reported the arrival of "le Frigorifique," until 1914, when landlords debated the advantages and disadvantages of using foreign labor. On the Gironde, see Loubère, *Red and the White*, 337–38. On the Mayenne, see Michel Denis, *Les Royalistes de la Mayenne et le monde moderne (XIX^e–XX^e siècles)* (Paris, 1977), 447–58; and on the Landes, see Suau, "La Gascogne landaise," 59–61. Equally important in other regions were the rise of monopolistic processors of grain, cheese, and sugarbeets, who were driving out small independent competitors. Harvey Goldberg, "Jaurès and the Formulation of a Socialist Peasant Policy, 1885–1898," *International Review of Social History*, 2 (1957): 388.

⁵¹ The *Ralliement* split the conservative vote by fostering the development of a conservative republicanism. See Bordes, "L'évolution politique," 21; Offen, "Paul de Cassagnac"; Siegfried, *Tableau politique*; and Denis, *Les Royalistes*, 447–58.

property at the end of the century. The Gers fell in 1892–1893 to well-organized republicans, who gained control of most of the department's municipal councils, the Conseil Général, and the department's five seats in the Chamber of Deputies. In their biggest victory, republicans elected an independent socialist, Thierry Cazes, to the Chamber from the electoral district of Lectoure in 1893. In 1897, local notables even lost their department's senatorial races, elections they thought they were guaranteed to win. Republican successes seem to have rested on the emergence of organized political groups affiliated with fledgling national political organizations, which sponsored electoral rallies to promote Radical and socialist candidates.⁵²

Similar changes began to occur in the Loire-Inférieure. Before the 1890s, all deputies elected from this solidly conservative bastion had held seats for at least ten years or were clearly on the right. Many were landlords; four had titles of nobility. Most were Catholic and Royalist, although some of them embraced Bonapartism. Eighteen of the fifty-one deputies elected in the Brittany departments were large landowners. However, in 1889, two left-leaning republicans won election in the Loire-Inférieure. In 1893, two additional republicans were elected, one of whom described himself as an independent socialist. In the 1898 elections, the anti-republican right regained a seat in the Loire-Inférieure, but the number of landowners among the Brittany deputies fell to nine out of forty-six. Lacking a candidate to run in the first electoral district of Nantes, a conservative coalition persuaded L.-M. Pucel to run as an anti-Semitic candidate, recognizing that at best he could generate a protest vote "for the good anti-Semites and the good French."⁵³

The Allier—once a stronghold of conservatives and Bonapartists—had also changed dramatically by 1898. The alliance between the rural and urban working class had become remarkably well knit by the end of the century and enjoyed great political clout. The Catholic editors of *La croix* wrote that socialist discipline and cohesiveness both impressed and terrified them in an 1895 Allier electoral campaign, although the department's two socialist factions voting together on the second round of elections still lost by a small number of votes.⁵⁴ Even where the socialists had yet to penetrate, left-leaning Radicals had decimated conservative opposition. Conservative landlords who ran as quasi-official candidates nevertheless remained a potent force in local politics, partly because of socialist factionalism.

Nationally, the 1893 legislative elections, the last held before the Affair, were a disaster for conservatives. Only fifty-eight Royalists and Bonapartists and thirty-six *Ralliés* were elected. As William Irvine argued, the right was reduced to "an

⁵² Bordes, "L'évolution politique"; Bordes, *Histoire de la Gascogne*, 331–84; and Wilson, *Ideology and Experience*, 223–24. On the deference paid to notables and priests in the Gers earlier, see Archives du Ministère de la Guerre MR 2280, "Mémoire sur le région d'Auch, 1822," and "Mémoire sur la route d'Auch à Toulouse, 1862."

⁵³ While this Nantes district contained a large part of the city, it also included an important viticultural region. *La croix nantaise*, February 6, 1898–June 5, 1898, especially May 5, 1898. See also Pascal, *Les députés*, 337–83.

⁵⁴ *La socialiste de l'Allier*, November 10, 1895; Derruau-Boniol, "Le socialisme dans l'Allier"; and Nancy Fitch, "Class Struggles in the Countryside: Social Change and Politics in Central France, 1200–1914" (Ph.D. dissertation, University of California, Los Angeles, 1985).

inconsequential rump in a chamber now dominated by the Opportunists [republicans]."⁵⁵ In short, the personal politics of large landowners and their representatives came under attack from men beginning to identify with nascent political parties. The parties were learning how to use the techniques of political rallies, mass-circulation newspapers, and campaign tactics pioneered by the Democratic-Socialists in 1849 and honed by the Boulangists in 1889. Thus armed, the fledgling parties began to challenge the impressive political machines of justices of the peace, priests, rural police, and other bureaucrats.⁵⁶ The political crisis for local notables coincided with an acute economic crisis generated by natural disasters combined with international agricultural competition.

By 1898, several socialist factions loosely organized into political parties gained national prominence with their persuasive Marxist explanations of the economic crisis. They predicted an increasing concentration of capital and productive activity in agriculture as had occurred in industry. They effectively won over many urban workers, but their most impressive gains were in rural departments. Peasants read socialist and other political newspapers, attended political meetings, wrote letters to the editors of newspapers, and began to write their own political tracts.⁵⁷

But, in 1898, socialists found it difficult to appeal simultaneously to their heterogeneous electorate, especially because of the divisions within the movement. By fixing the price of bread and meat, socialist municipalities fed the working class while economically squeezing bakers and butchers. While few urban workers objected to increases in the taxes on rural agricultural goods (*octrois*) to pay for social programs, the shopkeepers, artisans, and peasants in smaller towns and villages suffered. Most artisans wanted lower prices for articles of consumption, while small proprietors wanted higher prices for wine and grain. Shopkeepers, artisans, and peasants all remained generally skeptical of socialist plans for the eventual collectivization of the means of production and forced the socialist factions to reformulate their agrarian and commercial policies.⁵⁸

However, the republican Opportunists and Radicals—the parties most associated with the rural vote—had little more to offer bakers, butchers, and peasants. In their most significant move, they engineered the approval of the series of tariffs associated with the government of Jules Méline. Their major goal was to keep foreign grain out of the country, a policy that largely benefited only the most substantial commercial producers, for peasants, especially vinegrowers, rarely

⁵⁵ William Irvine, *The Boulangier Affair Reconsidered: Royalism, Boulangism, and the Origins of the Radical Right in France* (New York, 1989), 165.

⁵⁶ An excellent example of the impact of the new politics can be found in Roland Treppe's exposé of the actions taken by the marquis de Solages to defeat Jaurès in 1898. Simply stated, the marquis was forced to adopt the campaign strategies of his opponents in order to win. See Roland Treppe, "Un campagne électorale étudiée d'après les archives privées," *Actes du 82^{me} Congrès National des Sociétés Savantes, Bordeaux, 1957* (Paris, 1958), 471–90; Bordes, "L'évolution politique."

⁵⁷ *La socialiste de l'Allier*, January 3, 1897; Pierre Brizon, "Le monde rural en Bourbonnais," *Le mouvement socialiste*, 158 (July 1, 1905): 353; ADA T.912, "Listes des vendeurs de journaux dans l'Allier, 1893"; ADA 4M, non-classé, "Le sous-préfet de Gannat au préfet, 25 juin 1879"; Bordes, "L'évolution politique"; *La socialiste de l'Allier*, January 3, 1897; Magraw, *France 1815–1914*, 338. Compare Burns, *Rural Society*, 131–36, esp. 134.

⁵⁸ See *La socialiste de l'Allier*, 1895–1900, especially February 2, 1896, and July 4, 1897; *La démocratie du Centre*, May 2, 1898.

marketed—and indeed often bought—grain. Moreover, other countries reduced their demand for French products in retaliation. While imports fell 13 percent, exports fell 27 percent in the decade of 1886–1896, hitting wine, champagne, and brandy producers especially hard.⁵⁹ The Opportunists and Radicals had difficulty explaining to their constituencies why they could not reverse the economic crisis and why they supported imperialistic adventures in Indochina and Africa. Some opponents attributed the Radicals' failures to laziness, others to their association with "cosmopolitan" (Jewish) rather than local interests. The idea that the Radicals principally promoted cosmopolitan interests was reinforced by the visible support prominent Radicals like Georges Clemenceau gave to Dreyfus.⁶⁰

The Dreyfus Affair thus came into the countryside in the midst of a fluid political environment integrally related to developments in commercial agriculture that upset the social and economic worlds of both small and large producers. By the 1890s, the right had begun to see the need to adapt to mass parliamentary politics, if only to elect leaders able and willing to restructure the French state along authoritarian—if not agrarian—lines. At the same time, the socialist left had begun to see electoral politics as an avenue toward their eventual triumph. Both right and left increasingly viewed their political vitality as dependent on winning elections, that is, on appealing to the masses.

AN ANALYSIS OF RHETORIC IS HELPFUL IN UNDERSTANDING how and why candidates used anti-Semitism to appeal to the masses. This rhetoric was perhaps sharpest in the Gers, where conservatives faced fierce competition. Four out of five victorious candidates from this department campaigned on anti-Semitic themes. The fifth joined the anti-Semitic bloc in parliament after the election. Furthermore, the election of the Gers deputies in 1898 hinged on rural issues that were incorporated into a new rhetoric of anti-Semitism. With one exception, none of the victors was an incumbent. None won on the first round of elections, and most won by only small margins. Thus none of these politicians was in control of a rotten borough, in which the majority of the population simply voted for the local notable without regard to what he advocated.

Faced with the bankruptcy of traditional conservative politics—a politics that, while varied, usually promoted authoritarian leadership—several Gers candidates rode to victory by appealing to an ancient stereotype in a new way.⁶¹ Two of the

⁵⁹ Lebovics, *Iron and Wheat*; Eugene Golob, *The Méline Tariff* (New York, 1944); Michael S. Smith, *Tariff Reform in France, 1860–1900: The Politics of Economic Interest* (Ithaca, N.Y., 1980); and Lhomme, "La crise agricole."

⁶⁰ This is an interpretation that comes from reading a variety of popular newspapers published in opposition to the Radicals at this time.

⁶¹ In one paragraph, Burns acknowledges that three (rather than five) anti-Semitic deputies were elected in the Gers. He names only Joseph Lasies. He adds that "the appeal [of the anti-Semites] was not limited to the Gers," then cites some other examples from the Isère and the Loir-et-Cher. Without additional evidence or explanation, he ends this section with the conclusion that, in spite of their appeal, the anti-Semites were not successful because they did not accompany their appeal with concrete action, nor did they have any significant organization in the countryside. He does not analyze elections and offers no explanation why voters in one of his two fundamentally agricultural case studies, the Gers, elected "three" anti-Semitic deputies, except to say that "local notables

Gers anti-Semites, Jules Delpech-Cantaloup and Joseph Lasies, ran on new or revived Bonapartist platforms. Two others, the Radicals Paul Decker-David and André Delieux, seemed to have been compelled through the process of campaigning to invoke anti-Semitism. The fifth victor, the Bonapartist journalist Paul de Cassagnac, did not campaign on any issue but joined the group of declared anti-Semites elected to the Chamber of Deputies in June 1898. Although he had earlier argued for the revision of Dreyfus's verdict, Cassagnac was not hesitant to appeal to anti-Semitic voters in 1898, once he recognized how useful anti-Semitism could be to the authoritarian nationalist politics he advocated.⁶²

An examination of electoral platforms helps reveal the basis of rural anti-Semitism.⁶³ Decker-David promised "severe repression of cosmopolitan Jewry," and Delieux assured voters that he stood against cosmopolitan speculators—large financiers, whether Jewish or others, "who steal from us." "To them," he promised his rural constituency, "war without mercy! They are more dangerous than phylloxera, hail, or floods."⁶⁴ Saint-Clar landowner Delpech-Cantaloup, a leading member of the local agricultural society, ran in one of the toughest campaigns of the election against the department's first socialist incumbent. He promised to eliminate property taxes and the city tolls (*octrois*) and opposed a progressive income tax. He also argued for a reorganization of the Chamber of Deputies that would enhance the representation of agricultural interests. Although he promised to fight for freedom of religion, the press, and speech, he added, "But I do not confound liberty with license," and told voters that he was deeply offended by those who insulted France by insulting the army in the defense of a traitor. If elected, he promised to oppose efforts to reopen the case against Dreyfus.⁶⁵

The most significant of the anti-Semitic deputies was Bonapartist Joseph Lasies, mayor of the village of Mormès and defender of viticultural interests. Lasies was

pressured small landowners, sharecroppers, and day laborers." Burns, *Rural Society*, 158–60; Bordes, "L'évolution politique"; Bordes, *Histoire de la Gascogne*, 331–84.

⁶² Cassagnac switched sides sometime between 1897 and June 1898. His newspaper, *L'autorité*, was "generally regarded as a supporter of the anti-Semitic cause" after 1898. His 1898 diatribes against Jews are well documented in François Bournand, *Les Juifs et nos contemporains* (Paris, 1898), 29, 105, 108–10. These diatribes included Cassagnac's suggestion that Jewish wealth be expropriated because "public opinion would regard it as an act of legitimate reprisal." He is typical of many agrarian notables who possessed fiefs in the provinces but lived in Paris. He left his estates in the hands of others who also managed local politics. See Wilson, *Ideology and Experience*, 207, 215, 222–25, 227–28, 383; Bordes, *Histoire de la Gascogne*, 362. Compare Offen, "Paul de Cassagnac," esp. 376–78, on Cassagnac's complicated relationship with Edouard Drumont.

⁶³ The analysis of electoral platforms is not necessarily the best way to tackle the problem of anti-Semitism in French politics. Ideally, one would want to look at the socioeconomic basis of anti-Semitic victories, but such an analysis would reveal very little about the Gers elections since the department was homogeneous. At the end of the nineteenth century, 72 percent of the population had agricultural occupations and 87 percent of the population lived in rural areas. Moreover, most of the Gers' fourteen "cities" had populations that barely exceeded 2,000. Statistically, in other words, the relevant explanatory variables did not vary. I have analyzed elections statistically in the department of the Allier, where such an analysis was necessary and meaningful. See also Bordes, *Histoire de la Gascogne*, 381–82; Courtes, "La Gascogne orientale," 209–10.

⁶⁴ France, *Programmes*, 1898, 277–78, 282–83.

⁶⁵ France, *Programmes*, 1898, 280–82.

elected on the second round in the electoral district of Condom, in the heart of Armagnac. He ran as an anti-Semitic candidate, complaining,

For twenty years, the current regime, hypocritically decorated with the name "the Republic," has deceived us and exploited us to the profit of an odious Jewish-parliamentarian bourgeoisie which, assisted with deceitful promises, has followed only one objective: to empty our pockets and to fill their own. It will not be enough to change men. It is necessary to transform this impotent regime, which, thanks to every bad instinct and every base passion, is nothing more than a vulgar syndicate of exploitation led by large cosmopolitan speculators, clothing with its highest protection Jews and the bloated [*repus*] while tyrannizing poor devils who suffer and work.⁶⁶

Lasies wanted to carry out a Bonapartist-like project based on a directly elected authoritarian leader. He reminded voters that liberty was impossible without responsible authority. "With the authority based on the suffrage of the entire nation," he argued, "we will have the necessary force to get rid of the Jews and their servants, who for twenty years have never ceased to violate our liberties, waste our money, and sully our national honor. We will become our own leaders and will become their master."⁶⁷ Like Delpech-Cantaloup, Lasies wanted to reorganize the Chamber of Deputies based on professional representation.

The appeal of rural anti-Semitism, simply stated, took two forms. Radicals like Decker-David and Delieux tended to focus on Jews as financiers. Since the number of Jews in the Gers could be counted on one hand, no one actually thought they were losing their property to Jews or even to Jewish usurers.⁶⁸ Rather, the Radicals singled out Jews as cosmopolitan, part of a worldwide force as threatening as the commercial agriculture of the United States. Symbolizing the threat to peasant property and the forces that produced the uncertainty of late nineteenth-century social change, Jews were more dangerous than pestilence, hail, or floods. Never mind that Jews were not generally involved in commercial agriculture. People understood them as wanderers or strangers, as symbols of uprootedness and uncertainty.⁶⁹

Moreover, Eugen Weber has argued, Jews were so often associated with usury in popular culture that "semantic confusion" worked to identify all economic enemies and exploiters as "Jews," despite their actual racial and ethnic identities. Nancy Green has shown that, in the nineteenth century, *juiverie* came to mean any kind of scandalous behavior, including usury and parasitism. *Juiverie* also came to be the word used to describe all foreigners.⁷⁰ In case readers missed the point of these constructed synonyms, *La croix* consistently reinforced the notion that Jews artificially kept prices low and encouraged excessive subdivision of property for their own purposes, both of which were practices that contributed to the small

⁶⁶ France, *Programmes*, 1898, 278–80.

⁶⁷ France, *Programmes*, 1898, 278–80.

⁶⁸ Phyllis Cohen Albert, *The Modernization of French Jewry: Consistory and Community in the Nineteenth Century* (Hanover, N.H., 1977), 319.

⁶⁹ Crates [Francis Delaisi], "Le camp capitaliste," *Revue critique*, 565, cited in Mazgaj, *Action Française*, 138.

⁷⁰ Weber, "Reflections on the Jews in France," 12–13; Nancy L. Green, "Socialist Anti-Semitism, Defense of a Bourgeois Jew and Discovery of the Jewish Proletariat: Changing Attitudes of French Socialists before 1914," *International Review of Social History*, 30 (1985): 381.

proprietor's inability to survive. In the words of Edouard Drumont, "the Jewish millionaires are squeezing the little French peasant in a vise."⁷¹ The Jew, in other words, became a convenient scapegoat that allowed the petite bourgeoisie and small peasant proprietor to attack the rich, especially those identified with international financial and agrarian capitalism, without identifying themselves with either the working class or the socialists. The demand to abolish usury was not new, but only at the end of the century did peasants and Radicals couch the demand in anti-Semitic political rhetoric.

The construction of the ideology employed by Decker-David and Delieux arguably came from the press of the period. "Le sans-patrie," a pamphlet warning France of the danger of cosmopolitan Jews and Freemasons, circulated widely throughout the Gers.⁷² More significant, *La croix* in particular published cartoons and articles promoting the idea that Jewish speculators threatened local agriculture. One cartoon published just before the May 1898 election and captioned "The Big Pirate" suggested that a Jewish merchant based his speculation in the bread market on his manipulation of the Spanish-American War in the Philippines. (See Figure 3.) The simple image links the outsider, the Jew, who is involved in the imperialistic adventures of another country, the United States, to a concrete situation (the price of bread) every peasant understood. While many peasants knew that the price of bread was higher in 1898 than it had been, some also attributed the higher prices to the war, which had cut off American wheat.⁷³ The cartoon suggests that prices depended on speculation in international or cosmopolitan affairs, that is, on undesirable behavior, *juiverie*. Several years earlier, socialist Jean Jaurès had taken a strong stand against international grain speculators in the influential regional paper, *La dépêche de Toulouse*. Coincidentally, one of the major speculators he identified was a merchant family named Dreyfus.⁷⁴

Making direct connections between its anti-Semitic themes and individual political candidates, *La croix du Gers* accused Delieux of being "fond of Zola and his Jewish protégé Dreyfus."⁷⁵ It also pointed out that Olivier Bascou, the incumbent deputy running against Cassagnac, was the son-in-law of a wealthy Jewish banker. The newspaper argued that it was necessary to vote out Bascou, since "one cannot have a whole population represented by a Jew."⁷⁶ A careful reading of the paper indicates that its editors believed the outcome of all the electoral campaigns in the Gers to hinge on people's feelings about the Jewish question.⁷⁷

⁷¹ Edouard Drumont, *La dernière bataille: Nouvelle étude psychologique et sociale*, as cited in Wilson, *Ideology and Experience*, 268. See also Sorlin, *La croix*, 108, 179.

⁷² *La croix du Gers*, May 1, 1898.

⁷³ *La croix du Gers*, May 8, 22, 1898. See also *La démocratie du Centre*, May 3, 1898.

⁷⁴ Dreyfus, the accused, is not obviously related to the family of grain merchants. There was another "Dreyfus Affair," involving the crash of Jewish banker Auguste Dreyfus's Peruvian guano business with the outbreak of war between Peru and Chile. This Dreyfus was accused of bilking thousands of people out of their savings and of profiting from the adventure to buy up large vineyards near Perpignan in southern France. Goldberg, "Jaurès," 385 n; APPP 1054, "Manifestations diverses à la suite du procès Dreyfus-Esterhazy," which mistakenly contains the dossiers on Auguste Dreyfus and the guano scandal.

⁷⁵ *La croix du Gers*, February 27, 1898.

⁷⁶ *La croix du Gers*, March 13, 1898.

⁷⁷ *La croix du Gers*, January–June 1898.



LA GRANDE FLIBUSTE

— *Faut-il encore augmenter le pain ?*
 — *Attends que j'aie compté les coups de canon tirés à Manille !*

FIGURE 3: "The Big Pirate" appeared in *La croix* on May 5, 1898, and in *La croix du Gers* on May 8, 1898. It portrays a man caricatured as a Jew, apparently involved in some kind of business. He asks over the phone, "Is it necessary to raise the price of bread again?" He is told, "Wait until I have counted the rounds of fire in Manila!"

In response to the charges raised in *La croix du Gers*, Delieux published several articles in *La fraternité* explaining his position on the Jewish question.⁷⁸ In one article, he acknowledged the importance of attacking Jewish bankers but also pointed out that Catholic bankers speculated, too. In another, Delieux assured voters that he believed Dreyfus was guilty. In yet a third, he asked, "Have we begun to have enough of the Dreyfus Affair? At your house, in the street, in the café, in the city, in the country, there is no other question than that."⁷⁹ At the same time, rumors circulated in opposition papers that Decker-David was a Prussian Jew, since his given name was Paul Decker, which could sound German or Jewish. The deputy's father published letters in many supportive newspapers, protesting

⁷⁸ The paper was edited by the Radical senator from the Gers, Paul Destieux-Junca.

⁷⁹ *La fraternité*, January 23, 1898, January 27, 1898, February 24, 1898.

that he was neither German nor Jewish. Decker-David campaigned for tariffs on foreign wheat and suppression of taxes, just as he had in his successful campaign of 1893. The only difference was his vow to repress "cosmopolitan Jewry," a likely response to the rumors and one compatible with his protectionist program.⁸⁰

Gers Radicals, however, were not merely responding to the anti-Semitic charges of their opponents. They, too, were willing to employ anti-Semitic rhetoric when they believed it would help them win elections. This is most evident in the campaign pitting the Radical incumbent Bascou against the former Bonapartist Cassagnac. Like *La croix du Gers*, *La fraternité* attacked Jewish high finance and supported its own candidate, J.-J. Dourrieu, against both Bascou and Cassagnac. To win votes for Dourrieu, it stressed the fact that Bascou was the son-in-law of a prominent Jewish banker. In the newspaper's words, "in this climate, it is sufficient to speak of the Jews" to win elections.⁸¹

Little evidence exists that the anti-Semitic attack helped Dourrieu, but there is no question that it hurt Bascou. When the previously popular deputy tried to explain to rural voters that his program was identical to that for which they voted in 1893, they silenced him with the chants "Down with Bascou! Down with the Jews!" In May, the Radicals associated with *La fraternité* paid for their anti-Semitic campaign, when Dourrieu lost and Bascou was forced into a runoff election with the anti-republican Cassagnac. Although the newspaper now tacitly supported Bascou, the damage was done. Cassagnac agreed to debate Bascou, but he found he had to say very little, for *La fraternité* had already created the climate he needed to win. Again Bascou was silenced in rural villages, this time with the chants "Down with the Jews! Throw the Jews out! . . . Down with the Prussians! Long live Cassagnac!" In his victory speech, Cassagnac acknowledged the significance of anti-Semitism in his triumph by 1,164 votes. To those who chanted, "Down with the Jew!" he wrote, "Thank you, thank you again!"⁸² After his defeat, Bascou publicly blamed his loss on *La fraternité's* anti-Semitic attack and challenged the newspaper's editor to a duel.⁸³

The Bonapartists and Royalists developed a different rhetoric of anti-Semitism. Although some Royalists such as Albert de Mun employed similar anti-capitalist, anti-Semitic rhetoric, many conservatives, including Delpech-Cantaloup of the Gers, le comte de Lévis-Mirepoix of the Orne, and Fernand de Pontbriand of the Loire-Inférieure, were reluctant to argue that the Jewish problem was one of a wealthy elite.⁸⁴ Over time, however, increasing numbers of conservatives and conservative republicans began to believe that anti-Semitism would enhance their appeal in rural areas, since verbal attacks on Jews were becoming popular.⁸⁵

⁸⁰ Bordes, *Histoire d'Auch et du pays d'Auch* (Roanne, 1980), 185; France, Chambre des Députés, 6th Législature, Session Extraordinaire de 1894, *Programmes professions de foi et engagements électoraux de 1893* (Paris, 1894), 337–38; *La fraternité*, April 3, 1898.

⁸¹ *La fraternité*, February 24, 1898.

⁸² *La croix du Gers*, June 5, 1898.

⁸³ Bascou wounded Destieux-Junca in the duel, sending the editor to the hospital. *La fraternité*, April 7–June 12, 1898; *La croix du Gers*, May 10–June 5, 1898.

⁸⁴ See the electoral platforms of these candidates reprinted in France, *Programmes, 1898*; Burns, *Rural Society*, 108–09; Irvine, *Boulangier Affair*, 216 n; Wilson, *Ideology and Experience*, 225, 270.

⁸⁵ Reports from the secretaries of the Indre and the Landes, April 1893, Archives de la Maison de France, AN, as cited in Irvine, *Boulangier Affair*, 171.

Believing that peasants despised Jews, many landed conservatives and their representatives began to make anti-Semitic arguments when justifying their political ideas. Frustrated over the debates that slowly produced the Méline tariffs that protected agriculture only after many were already ruined, these men wanted to replace parliamentary rule with rule by corporate bodies organized around economic interest groups. For Lasies, this meant getting rid of the Jewish-parliamentarian bourgeoisie.

Significantly, both Delpech-Cantaloup and Lasies coupled anti-Semitism with anti-republicanism. Many Jews were intellectuals, professors, lawyers; some were orators; all were viewed as part of the retinue of parvenus whom Karl Marx had identified with the politics of high finance and large-scale industry in his *Eighteenth Brumaire of Louis Bonaparte*.⁸⁶ Moreover, in the anti-Semites' view, Jews were a people without a country, who threatened France by their cosmopolitan nature. Popular culture, the church, and religious publications also kept alive the notion that Jews were a race of thieves.⁸⁷ One small phrase thus joined them to the Republic, the impotent state that stole from everyone by pocketing excessively large tax revenues.

The rhetoric of these men identified the entire Jewish community with financial and industrial capital. The proto-Zionist Bernard Lazare explained it this way: "landed capital, in its struggle with industrial capital, has become anti-Semitic, because the Jew is, for the landed proprietor, the most typical representative of commercial and industrial capitalism."⁸⁸ Anti-Semites found it easy to single out the Rothschilds, as bankers, and Jewish financiers associated with the disastrous Panama Canal scandal, although the real culprits of the scandal were not Jewish. When Delpech-Cantaloup and Lasies argued for the reorganization of parliament around economic interest groups that would increase agricultural representation, they implicitly identified their political and economic enemies as financial and industrial capitalists, that is to say, as Jews. The Jew became a convenient scapegoat that allowed the large landowner to attack a certain class (financial capital) without directly threatening a very fragile and probably fictive alliance between agrarian and industrial magnates to counter the threat from below.⁸⁹

Conservative arguments also were cast in the language used in newspapers. However, the association of Jews with republicanism and the parliament is much more complex than the linguistic and symbolic equation between "Jew" and "capitalist." Once again, *La croix* took the lead in using anti-Semitic vocabulary and

⁸⁶ Most Jews, of course, were not financial and industrial capitalists. See Michael Marrus, *The Politics of Assimilation: A Study of the French Jewish Community at the Time of the Dreyfus Affair* (Oxford, 1971); Paula E. Hyman, "The French Jewish Community from Emancipation to the Dreyfus Affair," in Kleeblatt, *Dreyfus Affair*, 25–36; Nancy L. Green, *The Pletzl of Paris: Jewish Immigrant Workers in the "Belle Epoque"* (New York, 1986); Nancy L. Green, "The Contradictions of Acculturation: Immigrant Oratories and Yiddish Union Sections in Paris before World War I," in Malino and Wasserstein, eds., *Jews in Modern France*, 54–77; Green, "Changing Attitudes of French Socialists"; Karl Marx, *Eighteenth Brumaire of Louis Bonaparte* (1852; New York, 1963); and Sorlin, *La croix*, 101.

⁸⁷ Joseph Reinach, *Histoire de l'Affaire Dreyfus*, quoted in Wilson, *Ideology and Experience*, 4.

⁸⁸ Bernard Lazare, *L'antisemitisme: Son histoire et ses causes* (Paris, 1934), 187; Wilson, *Ideology and Experience*, 272–79.

⁸⁹ Lebovics, *Iron and Wheat*.



TIRAGE AU SORT

— J'ai le n° 22, Monsieur le sous-préfet.
 — Pon bour drois ans de serfice; Fife la Vrance!
 — Ben, il parle comme un youpin not sous-préfet

FIGURE 4: "The Draft Lottery" appeared in *La croix du Gers* in January 1898. A French peasant draws his number and says to a man caricatured as a Jew, "I have number 22, Monsieur Subprefect." The subprefect responds in a German-Yiddish accent, "Good for three years of service; long live France!" The potential draftee then exclaims, "Ben, he speaks like a *youpin* [slang for a Jew], not like a subprefect."

images, this time to challenge the Republic. In January 30, 1898, a cartoon in *La croix du Gers*, for example, portrayed a young conscript drawing his draft number in front of three men, one of whom was caricatured as a Jew. (See Figure 4.) The caption reads, "I have Number 22, Monsieur Subprefect." The Jew responds with a German-Yiddish accent, "Pon bour drois ans de serfice; Fife la Vrance!" (Good for three years of service; Long live France!) The conscript then mutters to himself, "Ben, he speaks like a *youpin* [slang for Jew], not like a subprefect."⁹⁰ The implication is that Jews have penetrated the bureaucracy throughout the provinces and are now in charge of obtaining draftees for the army. Not only does this image question the integrity of the French army but it also makes the point by demonstrating the impact of the "Jewish Problem" on ordinary peasants. Virtu-

⁹⁰ Pointing to the significance of language in the construction of the ideology, many anti-Semitic cartoons were captioned phonetically to indicate a thick German-Yiddish accent. The cartoon is from *La croix du Gers*, January 30, 1898. See Burns, *Rural Society*, 211 n; Hyman, "Dreyfus Affair," 94.

ally nowhere in popular memory was the “tyranny” of the state over peasants greater than in its conscription of rural sons. Language is also used in this cartoon to identify the Jewish Frenchman as an impostor. Although most Jews were completely assimilated and spoke perfect French, *La croix* regarded them as unassimilable.⁹¹ Another cartoon portrayed snakes with Jewish heads biting French soldiers, while still another, which appeared next to “The Big Pirate” on May 5, suggested that only Jews participated in elections, since Jews manned the polls, while most French abstained from voting. (See Figure 5.)

What can be called the nationalist construction of French history at the time was summarized in a poster that appeared in 1902, “The History of a Politician.” (See Figure 6.) In this narrative account, the politician, though exhibiting Jewish features, is educated in a seminary. He becomes a Freemason and then wins election by promising “more butter than bread.” Jewish financiers bribe him to vote the right way during the Panama Canal scandal. He encourages workers to strike against their bosses and accepts money from the “Dreyfus syndicate” to malign the army. In the end, it is up to “the People” along with the nationalists to sweep the politicians and their friends into Paris’s sewers, where they belong. The idea echoed Cassagnac, who once wrote, “A well-led battalion admirably supplements the weaknesses of the Constitution. Let us clean out everything; *let the broom become a symbol!*”⁹²

Both Delpech-Cantaloup and Lasies offered a solution: get rid of the Jews and create a government of corporate bodies under the control of a directly elected authoritarian leader. As Lasies had argued, it was no longer sufficient to change men. Earlier in the century, Bonapartists and Boulangists had also promised government through a strong leader and an authoritarian revolution. Yet, while the Bonapartists achieved widespread success during the Empire, they had been unable to attract votes in the 1890s. And, while the Boulangists succeeded in many parts of France, it was Cassagnacisme, not Boulangism, that dominated the elections of 1889 in the Gers. The earlier triumph of Cassagnacisme depended on the continuation in the Gers of a system of official candidacy pioneered by the July Monarchy and refined by the Second Empire. The prefects and subprefects controlled politics by purging republican mayors and teachers, closing republican

⁹¹ *La croix nantaise*, for example, wrote that it was important to treat Jews as human beings, but “the Jews are an unassimilable people and because they are unassimilable, we must refuse to give them the privilege or favor of naturalizing them”; February 6, 1898. As evidenced in the works cited in note 85, the issue of Jewish assimilation in *fin-de-siècle* France is complicated. Most Jews were at least institutionally assimilated into French society, although their consciousness of being Jewish persisted. In fact, *fin-de-siècle* anti-Semitism can be interpreted as a reaction against the social integration of Jews into French society. On the other hand, an increasing number of Yiddish-speaking Jews from Eastern Europe as well as Jews from Alsace-Lorraine were swelling the Jewish population of Paris at this time. But these Jews were ethnically distinct, were generally poor, and joined the French proletariat, not the French administration. See Green, *Pletzl of Paris*; Jay R. Berkovitz, *The Shaping of Jewish Identity in Nineteenth-Century France* (Detroit, Mich., 1989); and Vicki Caron, *Between France and Germany: The Jews of Alsace-Lorraine, 1871–1918* (Stanford, Calif., 1988).

⁹² My emphasis. René Rémond, *The Right-Wing in France from 1815 to De Gaulle*, 2d edn., James M. Laux, trans. (Philadelphia, 1966), 224. The last frame of the poster is nearly identical to a cartoon published by Bobb in *La silhouette*, February 20, 1898. The cartoon is reprinted in Grand-Carteret, *L'affaire Dreyfus et l'image*, 121.



FIGURE 5: "No Abstentions." This cartoon appeared in *La croix* on May 5, 1898, suggesting the unlikely situation of a polling place dominated by Jews. The man guarding the ballot box says to the Jewish voter, "You are going to vote, are you not, Master Jacob? It is only the French who abstain!"

clubs, and delivering patronage in the form of roads and jobs. But, by 1898, this system of official candidacy had collapsed, largely as a result of the influences of republican organizations and the popular press.⁹³

Without anti-Semitism, neither Royalism nor Bonapartism nor Boulangism, nor any of their radical right transformations, could continue to develop more than a limited popular appeal in the countryside. In the long run, they could not stop the spread of the international capitalist development ruining small producers and large landowners, nor could they offer much to agricultural or industrial workers. Traditional conservatism, despite its form, could work politically only where patronage politics and electoral fraud prevented the operation of disci-

⁹³ Offen, "Paul de Cassagnac," 179, 341; Burns, *Rural Society*, 101–05; Bordes, "L'évolution politique."



FIGURE 6: This poster, which appeared in 1902, depicts the "History of a Politician." It brings together in a single document many of the themes raised in the popular press during the Dreyfus Affair. Courtesy of the Houghton Library, Harvard University.

plined political parties. At the end of the nineteenth century, the new radical right recognized the impossibility of seducing voters with the benefits of a pretender or a Bonaparte and could define itself only through opposition; anti-Semitism

became its means of distinguishing all that was Other.⁹⁴ The prefect of the Landes told why Théodore Denis, the “republican nationalist clerical” of Dax and a right-wing “Jacobin,” won his election: “He gives satisfaction to all opinions and is the candidate of all parties. He has made a speciality of anti-Semitism . . . He loves the people and is loved by them. They’re his idol.”⁹⁵ Denis recognized sovereignty only in “the people,” who distrusted representative democracy as embodied in parliamentary republicanism and who believed that the parliament itself was foreign, that is, Jewish.⁹⁶ The same could be said of Delpech-Cantaloup, Lasies, and Cassagnac.

The effect was not limited to the southwestern departments of the Gers and the Landes. In much of France, the new nationalists argued for a “France for the French.” In the words of Fernand de Pontbriand of the Loire-Inférieure, the French as a race were threatened by the Jewish race, which treated France as a “conquered country.”⁹⁷ For Louis Le Roy de Loulay of the Charente-Inférieure, foreign Jews without a country were contaminating the army and promoting social disintegration.⁹⁸ Nationality to these conservatives no longer depended on geographical boundaries; it constituted a racial community. In the words of Royalist Count Edouard de Lur-Saluces of Bordeaux, “everyone has understood that we are struggling *against a race* and defending ourselves against its cosmopolitan power.”⁹⁹ This attitude is reflected in the cultural production of the period. Many images and artifacts drew from the repertoire of images used to represent collectibles from the Empire in museums and international expositions: glass paperweights containing Dreyfusard figures embodied as fetuses, posters representing Dreyfusards as monsters in a freak show. (See Figure 7.) In short, they resembled the caricatures used to represent those who were not French.¹⁰⁰

Dreyfus, the Jewish traitor, thus appeared almost miraculously in 1898 to save the cause of anti-parliamentarian conservatives.¹⁰¹ Everything in the traditional stereotype of the Jew—the stranger, the man without a country, the usurer—

⁹⁴ Iconographically, Jews were often embodied as Hottentots, as blacks, as the quintessential Other. Sander L. Gilman, “Black Bodies, White Bodies: Toward an Iconography of Female Sexuality in Late Nineteenth Century Art, Medicine, and Literature,” *Critical Inquiry*, 12 (Autumn 1985): 204–42; and Wilson, *Ideology and Experience*, 584–601. Gilman treats the theme in more depth in *Difference and Pathology: Stereotypes of Sexuality, Race, and Madness* (Ithaca, N.Y., 1985).

⁹⁵ Bordes, *Histoire de la Gascogne*, 377.

⁹⁶ The language is adopted from Karen Offen’s description of Cassagnac, in Offen, “Paul de Cassagnac,” 391. See also Steven Louis Englund, “The Origin of Oppositional Nationalism in France, 1881–1889” (Ph.D. dissertation, Princeton University, 1981), 570–88.

⁹⁷ In some places, candidates also invoked regionalism, that is, “Lozère for the Lozériens.” See France, *Programmes, 1898*, 414–15.

⁹⁸ France, *Programmes, 1898*, 149–50.

⁹⁹ Lur-Saluces to Duc d’Orléans, February 6, 1899, as cited in Irvine, *Boulangier Affair*, 173, Irvine’s emphasis.

¹⁰⁰ “Scheurer-Kestner Alchimiste,” a cartoon published by Bobb, *La silhouette*, November 7, 1897, reprinted in Grand-Carteret, *L’affaire Dreyfus et l’image*, 114; Carol A. Breckenridge, “The Aesthetics and Politics of Colonial Collecting: India at World Fairs,” in *Comparative Studies in Society and History*, 31 (April 1989): 195–216; Hyman, “Dreyfus Affair,” 100.

¹⁰¹ Anti-Semitism surfaced in Boulangism, but it lacked unifying power as an ideology before the Dreyfus Affair. Boulanger himself was not an anti-Semite. Anti-Semitism also surfaced in the discourse of those who opposed Boulangism. See Marc Angenot, *Ce que l’on dit des Juifs en 1889: Antisémisme et discours social* (Saint-Denis, 1989); Zeev Sternhell, “The Roots of Popular Anti-Semitism in the Third Republic,” in *Jews in Modern France*, 104–07; Irvine, *Boulangier Affair*, 157–82; and Burns, *Rural Society*, 114–15.

Price: 10 centimes **EXPOSITION DES HORREURS**...Image Populaire N. 5



FIGURE 7: "The Freak Show." This poster is an example of the way in which Dreyfus and his supporters were embodied as freaks alongside of exotic animals usually found only in the colonies. Jews, Radicals, and socialists in the poster are thus linked to colonial images. In contrast to those pictured on the border, well-dressed and well-formed Frenchmen, including the prominent anti-Semites Edouard Drumont and Paul Déroulede, occupy the center of the poster with a guillotine. They are promising to execute the freaks, clearly on the margin of the society defined by patriotism (*La patrie*) and the flag (*le drapeau*). The poster itself looks like a theater advertisement. Léon Hayard, who published the poster, also produced toys and other anti-Semitic cultural commodities. Courtesy of the Houghton Library, Harvard University.

could be incorporated into a new ideology linking popular anti-Semitism with hostility toward big merchants, big landowners, big bankers, and speculative capitalism in general. Collectively, these ideas eventually produced a popular conservatism grounded in nationalism without threatening the economic or class interests of its perpetrators. As social critic and Dreyfusard Daniel Halévy argued, anti-Semitism allowed conservatives to function politically without forming a true conservative party. More important, it enabled them to form an imaginary community with Radicals, nationalist *Progressistes*, urban nationalists, and some peasant proprietors with whom they otherwise had little in common. In the words of Gers Radical André Delieux, the formation of this imaginary community was “a bizarre phenomenon” in which “men who belong to the parties that are most opposed are thinking alike” because of “this unhappy Affair.”¹⁰² Newspapers and other cultural commodities were the expressions of this imaginary community and linked these diverse groups of consuming publics through the print market.¹⁰³

Given the weakness of organized political parties in their department, the Gers anti-Semites successfully used the press to channel peasant hatred and distrust of traditional enemies (landlords and the state) into hatred of the Jew. Many Gers peasants began to believe that parliamentary “Opportunists and Jews are responsible for the Panama Canal scandal, the expeditions into Madagascar, Tonkin, Dahomy,” that is, for everything wrong with France.¹⁰⁴

IF ANTI-SEMITES WERE SUCCESSFUL IN SOME DEPARTMENTS such as the Gers, they were remarkably unsuccessful—with a few exceptions—in others, the Allier at the foothills of the Massif Central, for instance. This is not to say that anti-Semitism did not enter the countryside in the Allier. In fact, the Dreyfus Affair became one of the largest issues in the 1898 campaigns there. Nevertheless, just because candidates argued that organized cosmopolitan Jewry represented the greatest danger faced by most peasants, and countless popular publications reinforced this position, peasants did not have to accept the argument. In the Allier, most did not.

In many respects, the Allier did not differ from departments where anti-Semites succeeded. There were no more Jews in the Allier than in the Gers. Like the departments in which anti-Semites succeeded, the Allier was rural, though it had a larger industrial population, a better transportation system, and fewer peasant proprietors. Most cultivators in the Allier were agricultural day laborers or sharecroppers. But, like the vinegrowers of the Gers, Allier vinegrowers had lost much to the phylloxera louse, and large landlords in the department were as affected by international competition as were those in the rest of France.

Not surprisingly, the Allier electoral district where anti-Semitism proved most successful was in the vinegrowing region of Gannat. There, the conservative

¹⁰² *La fraternité*, February 24, 1898. See also Daniel Halévy, *Apologie pour notre passé (1907–1910), Luites et problèmes* (Paris, 1911), 113.

¹⁰³ Benedict Anderson, *Imagined Communities: Reflections on the Origin and Spread of Nationalism* (London, 1983).

¹⁰⁴ Courtes, “La Gascogne orientale,” 216.

candidate, Alphonse Labussière, told voters in the viticultural village of Louchy-Montfand that it was useless to support a progressive income tax as a means of attacking Jews like the Reinachs and the Rothschilds, for a tax meant nothing to millionaires. His Radical opponent, Louis-Gabriel Delarue, disagreed on the value of a progressive income tax, but he agreed that Jews had too much power in the government and in the economy, an argument that won him much applause at rural rallies. In his official program, Delarue campaigned “to defend the national treasury against the speculating of cosmopolitan Jews, gamblers of high finance who enrich themselves by pillaging the small and middle classes.”¹⁰⁵ In the rural village of Monteignet, someone asked Delarue what he thought of the “Dreyfus syndicate.” After he promised he was “no friend of the Jews,” those present at this rally agreed to support him. In Saint-Bonnet-de-Rochefort, one of the more isolated rural villages of the department, another voter asked Delarue if he was part of “the Jewish syndicate.” And in Ebreuil, a rural market town, he won the support of voters who liked his positions on agriculture and on the Dreyfus Affair.¹⁰⁶ These meetings induced the *Journal de Gannat* to predict his victory, remarking that “he has fought energetically against the maneuvers and actions of cosmopolitan Jewry.”¹⁰⁷

In spite of the success of anti-Semitism in mobilizing voter turnouts in Gannat, not everyone accepted the anti-Semitic argument. Elsewhere in the Allier, the anti-Semites fared much worse. In the electoral district of Lapalisse, for example, they were forced to run against Jules Gacon, a popular and powerful Radical up for reelection. A dedicated republican, Gacon wanted tax reform, the creation of agricultural credit unions, the formation of mutual assistance societies, separation of church and state, and the continuation of free public education.¹⁰⁸ His was a program identified with Radicals everywhere in France, the program of peasants, artisans, and small shopkeepers.¹⁰⁹ Yet it was not his program that enemies openly opposed. When he appeared at a meeting on April 30 in Saint-Germain-des-Fossés, voters hooted him down by chanting “Long live the army!” “Down with Dreyfus!” “Down with the anarchists!” “Down with Jaurès’ gang!” Voters ended the meeting by endorsing his opponent, “the real republican” Paul Debray, who vowed “to silence the Dreyfusards.”¹¹⁰

Debray’s program was not ephemeral. He promised beleaguered peasants that he would improve the wealth of the countryside through patronage projects and that he would end the health regulations ruining the wine industry. But these issues barely surfaced in the newspapers that supported him. Large landowners, commercial farmers, and engineers like Debray had begun to join forces in the

¹⁰⁵ *Journal de Gannat*, April 3, 1898; France, *Programmes*, 1898, 26; Derruau-Boniol, “Le socialisme dans l’Allier.”

¹⁰⁶ *Journal de Gannat*, April 16, 23, 1898.

¹⁰⁷ *Journal de Gannat*, April 23, 1898.

¹⁰⁸ *L’indépendent*, May 3, 1898; *La démocratie du Centre*, May 3, 1898; France, *Programmes*, 1898, 27–28.

¹⁰⁹ Jean Viple, “Le radicalisme dans l’Allier sous la Troisième République,” *Notre Bourbonnais*, 9^{ème} série, no. 208 (2^{ème} trimestre 1979): 152–79; Jacques Kayser, *Les grandes batailles du radicalisme, des origines aux portes du pouvoir, 1820–1901* (Paris, 1962 [c1961]).

¹¹⁰ *L’indépendent*, May 3, 1898; and *La démocratie du Centre*, April 30, 1898. See also *La démocratie du Centre*, May 3, 1898; *Officiel Vichy*, April 28, 1898.

Allier as nationalists to protect the church, religious schools, and respect for the army, priests, and the aristocracy. To these people, Gacon symbolized everything that threatened the country and their families.¹¹¹ Gacon could have been the politician in the poster "The History of a Politician." His enemies accused him of having Jewish features, of being a Freemason, of pimping, of taking bribes from Jews. *La croix de l'Allier* argued that he represented the ideal of "Judeo-Masonic" politics. Indeed, Gacon invited the anti-Semitic attack on his candidacy when he gave a speech to new conscripts in January 1898 advocating reopening the case against Dreyfus and calling for army reform.¹¹²

So many anti-Semitic-related fights broke out at Gacon's electoral rallies that the subprefect of Lapalisse sent special gendarmes to each village to guarantee peaceful elections.¹¹³ Perhaps as a consequence, there was little violence on election day, and Gacon won by a large margin of votes on the first round. Much evidence suggests that hostile landowners and their tenants staged the electoral fights. Debray won a majority in only eight of seventy-seven villages, towns, and urban wards. The results of this election can lead to only one conclusion: in spite of some significant electoral demonstrations, anti-Semitism had little appeal in this part of the Allier. Statistically, as the percentage of forest workers and agricultural day laborers increased, so too did the percentage of the vote given to Gacon. Because only a small minority of electoral districts gave a majority to Debray, the rural proletariat was evidently well enough organized to reject a candidate identified with large landowners. Those who did vote for Debray tended to come from a handful of peasant-dominated viticultural villages in the hinterland of Vichy and in the recently drained and improved Forterre.¹¹⁴

The question, of course, is why anti-Semitic and anti-Dreyfusard rhetoric played a central part in these elections. Allier Royalists had begun to define a new political rhetoric in a *Le courrier* article of 1881. They polarized France into "the Great Notables" with their clients and "the new stratum" of men, parliamentary politicians and their followers, who drank, beat their wives, sang obscene songs, read newspapers, and insulted priests. In 1882, they moved in an implicitly racist direction by linking absence of faith in God to support of non-French interests when they ridiculed the atheist Victor Schoelcher as "the deputy of the Negroes" for his role in abolishing slavery in the French empire. Bonapartists soon took up

¹¹¹ ADA 3M 59 1898, "Elections législatives, divers, 1898"; and ADA 3M 61–65, "Elections législatives de l'Allier, 1902."

¹¹² ADA 3M 61–65; *Officiel Vichy*, April 28, 1898; *La démocratie du Centre*, April 30, 1898; *La croix de l'Allier*, December 19, 1897, February 6, 1898, March 13, 1898.

¹¹³ "Le sous-préfet de Lapalisse au préfet, 3 avril 1898," in ADA Allier, 3M 59 1898, "Elections législatives, divers, 1898."

¹¹⁴ Devastated by the loss of vines to phylloxera after 1895, the peasants in these villages and towns—more than others in the department—were also disrupted by *fin-de-siècle* social and economic change associated with the rise of Vichy as a major tourist center in central France. Debray did best in places with large proportions of *rentiers*, at least some of whom were large landowners, and *fermiers*, most of whom were commercial farmers. On the villages in the hinterland of Vichy, see Touret, "Les campagnes bourbonnaises," 90, 180. I compiled the data I used from manuscript censuses and election results. I ran an additional sample on data found in Secretin, "Le Boulangisme dans l'Allier." Our data is not comparable enough to run together, but the results from the two samples are generally consistent given our differences in coding and sampling. See also ADA Allier, Série M, non-classé, "Le sous-préfet de Lapalisse au préfet, 9 mai 1910"; and ADA M.105.c, "Syndicats agricoles, Enquête de 1910."

the theme, arguing that public schools without God would destroy respect for civilization and tradition. Subsequently, Royalists and Bonapartists collectively began to define a new right grounded in an extreme interpretation of what Christianity represented: tradition, hierarchy, and the “folk” (the authentic French). By 1898, they were defining all struggles in simplistic dichotomies: great notables versus the new stratum, Christians versus Jews, “folk” versus urban dwellers. As a consequence, they constructed a political discourse of anti-Semitism and issues related to Dreyfus that all newspapers and parties had to confront. Gacon was a particularly useful embodiment of the negative half of their dichotomies. A Radical who rejected older political organizations and ran on his own, he was also an anti-clerical Freemason who in their eyes looked Jewish. Moreover, Gacon sided with urban interests against abolition of the regulations destroying the wine industry.¹¹⁵ In the months preceding the election, *La démocratie du Centre* repeatedly pointed to Gacon’s positions on the negative side of the conservatives’ dichotomies.¹¹⁶

Accepting the notion that Jews threatened France, the Radical paper *L’indépendant* responded by publishing an editorial arguing that Gacon fought against “dirty” Jews and promoted viticultural interests. It also published an open letter to Paul Debray that denied any connection between Gacon and Jews or those in parliament who defended Dreyfus.¹¹⁷ The rhetoric from *La démocratie du Centre* nevertheless set the tone for the debate and ensured that, in spite of Gacon’s American-style campaign, he would be confronted with anti-Semitic remarks in every village he entered. According to published press reports, he was never able to discuss his platform, as he was shouted down by those who wanted to know only of his position on the Dreyfus Affair.¹¹⁸

The influence of the press emerges much more clearly in an examination of the elections held in the two electoral districts of Montluçon in the Allier.¹¹⁹ Several letters to the editors of *La socialiste de l’Allier* mentioned the increased frequency of newspapers during the elections. One voter from the village of Saint-Fargeol complained that the conservative candidate, Marcel Vacher, was inundating his village with Bonapartist newspapers, but the voter remarked that “unfortunately for Vacher, no one believes anything printed in them.”¹²⁰ The editors of *La socialiste de l’Allier* feared that readers were paying attention to the press and began to debate directly with rightist newspapers. On February 27, 1898, they reprinted an article from *La démocratie du Centre* arguing that the socialists had decided to oppose anti-Semitism. The article added that the collectivists were the Jews’ closest allies. The Guesdist candidate Paul Constans published an editorial next to

¹¹⁵ ADA 3M 59 1898, “Elections législatives, divers”; Lelong, “La presse bourbonnais”; “L’information dans la presse locale”; and “La presse bourbonnaise dans l’Affaire Dreyfus,” *Notre Bourbonnais*, 9^{ème} série, no. 207 (1^{ère} trimestre, 1979): A–D.

¹¹⁶ *La démocratie du Centre*, January–June 1898.

¹¹⁷ *L’indépendant*, May 7, 1898; April 29, 1898.

¹¹⁸ ADA 3M 59 1898, “Elections législatives, divers.”

¹¹⁹ The Bonapartist press also made anti-Semitic accusations against candidates in the two electoral districts of Moulins, but this had almost no impact. These elections are not analyzed here. *L’indépendant*, May 7, 1898.

¹²⁰ *La socialiste de l’Allier*, May 1, 1898. Vacher also gave away chocolate medallions to potential voters. See *La socialiste de l’Allier*, April 24, 1898, and May 5, 1898.

the reprint acknowledging that candidates were likely to be maligned but that he would never stand for the insult implied in *La démocratie du Centre*. "All of my life, all of my actions," he wrote, "protest . . . against this venal accusation." Refusing the right's opposition of French versus Jew, another article in the same paper pointed out that the Guesdist position was to stay out of the conflict, adding that the socialists were in favor of expropriating rich Jews like the Rothschilds as well as rich Catholics like Eugene Schneider.¹²¹

La croix and *La croix de l'Allier* responded by publishing a string of articles warning peasants that the socialists were merely following the orders of Prussian Jews, that France had been invaded by foreigners, and that Jews were creating monopolistic department stores and grocery chains that were ruining small producers. Quoting the Parisian *Le petit journal*, *La croix* concluded, "Dreyfus has become the flag, the stench, the God of everything we detest."¹²² *La socialiste de l'Allier* bounced back with a series of articles reprinting and then refuting what was published in *La croix de l'Allier*. It also printed news from other regional editions of *La croix*, paying particular attention to the activities of Gers candidate Cassagnac.¹²³

Partly because of the influence of newspapers, there were several anti-Dreyfusard riots in the district of Montluçon in 1898 (including one in the rural village of Ronnet). The major targets were small shops believed to be owned by Jews. Many of those attacked, however, wrote their own letters to newspapers with evidence certifying that they were not Jewish. Besides these incidents and the newspaper warfare, anti-Semitism played a very minor role in the Montluçon legislative elections of that year. Candidates from both sides implied that Jewish interests controlled their opponents, but few seemed very concerned.¹²⁴

Voters were more worried about which socialist faction (the Guesdists or the neo-Blanquists) would win and whether either faction would collectivize property than they were about the "Jewish problem." Based on the suffrage of miners, metallurgy workers, vinegrowers, agricultural day laborers, and sharecroppers, socialists from both factions had done well in most legislative elections held since 1889, and many peasants supported the neo-Blanquist candidate Christophe Thivrier, who shocked everyone by wearing a peasant smock when he took his seat in the Chamber of Deputies.¹²⁵

Other peasants began to worry about socialist successes, especially when candidates vowed to collectivize property. Constans, a local Guesdist, facetiously explained the impossibility of surviving as a peasant proprietor in a capitalist-dominated century. Chronicling the experiences of his close peasant relatives, he detailed the inevitable effects of debt, mortgage, and foreclosure on those who

¹²¹ *La démocratie du Centre*, February 27, 1898.

¹²² *La croix*, January 21, 1898. See also *La croix de l'Allier*, January–June 1898.

¹²³ *La socialiste de l'Allier*, January–June 1898. *Le tocsin populaire* also debated *La croix de l'Allier*. ADA M.290.c, "Coupures de la Presse."

¹²⁴ *La croix de l'Allier*, January–February, 1898; Georges Rougeron, *Le département de l'Allier sous la Troisième République* (Montluçon, 1965), 103; Fitch, "Class Struggle in the Countryside," chap. 8; Guy Rousseau, "Un miroir de l'action socialiste dans l'Allier: *Le tocsin populaire* de 1895 à 1898," *Notre Bourbonnais*, 2^{ème} série, no. 233 (1985): 69, 79 n; *La socialiste de l'Allier*, January–June 1898.

¹²⁵ Georges Rougeron, *Le mouvement ouvrier en Allier* (Moulins, 1981); Ernest Montusès, *Le député en blouse* (1978; rpt. edn., Roanne, 1983).

tilled the soil, and advocated collectivization of agriculture.¹²⁶ But most peasants rejected his analysis and feared his victory.

In many places, the threat of socialism undoubtedly contributed to the development of modern anti-Semitism. *La croix de l'Allier* argued that Jews and socialists were joined in opposition to the church, the army, and the family.¹²⁷ In local sharecropper Emile Guillaumin's fictional account of the rural left, *Le syndicat de Baugnoux*, the right-wing press wrongfully accused a socialist sharecropper of undercutting local traders by dumping Jewish products in his cooperative store.¹²⁸ Fear of socialism also may have been the central issue behind the attack on Gacon in Lapalisse, where hecklers associated Dreyfus and Gacon with anarchists and socialists. Here and elsewhere, many identified Jews with socialists, especially with Jaurès, who defended Dreyfus.¹²⁹ Both Jews and the socialists were cosmopolitan and international; both threatened the army; both contributed to the weakness and fragmentation of the Republic. The connection between Jews and socialists as international wanderers without countries was reflected in several popular prints, including Figure 8, which appeared in *Le grélot*.¹³⁰

Once again, the facts belie the connection. Until late 1898, most socialists demanded that the proletariat stand aside from the Dreyfus Affair, "the bourgeois civil war" designed to draw attention away from class struggle.¹³¹ Moreover, many socialists had previously made use of anti-capitalist, anti-Semitic rhetoric themselves.¹³² In fact, many Royalists were initially reluctant to exploit anti-Semitism as a political issue because of its association, prior to the Dreyfus Affair, with the extreme left.¹³³ By the mid-1890s, however, anti-Semitism was so strongly identified with anti-socialist politics and the politics of large landowners in the Allier that socialist leaders from both the neo-Blanquists and the Guesdists began to educate peasants, sharecroppers, and workers about the real enemies they faced. The local Guesdists blamed the anti-Dreyfusard riots in Montluçon on

¹²⁶ *La socialiste de l'Allier*, July 4, 1897.

¹²⁷ *La croix de l'Allier*, February 27, 1898.

¹²⁸ Emile Guillaumin, *Le syndicat de Baugnoux* (Paris, 1959), 169.

¹²⁹ *La croix du Gers*, May 22, 1898; *La croix nantaise*, May 15, 1898; France, *Programmes*, 1898, 192–97, 244–48. Evidence from Wilson, *Ideology and Experience*, 126 (on the village of Cogolin), used with data presented by Tony Judt, *Socialism in Provence, 1871–1914: A Study in the Origins of the Modern French Left* (Cambridge, 1979), 94, 128, 225–26, leads one to the same conclusion. See also the voided ballots included in official election returns for the canton of Lecture in AN C.5145, "Elections législatives, 1898, Gers."

¹³⁰ This cartoon appeared in 1894. Many similar examples are in *La croix*.

¹³¹ Green, "Changing Attitudes of French Socialists," is especially effective in making this point. See also *La socialiste de l'Allier*, May 19, 1895, January 23, 1898, January 30, 1898. Claude Willard, *Le mouvement socialiste en France (1893–1905): Les Guesdistes* (Paris, 1965), 410–40.

¹³² This is particularly true of *Le tocsin* and *Le tocsin populaire*, newspapers published by the neo-Blanquists in the Allier. Both reprinted articles by Drumont, although, significantly, they emphasized his anti-parliamentary arguments and rarely mentioned his views on the Jewish problem. Other articles attacked Jewish wealth; predicted that the Rothschild family would own all of France in fifty years; complained about the power of Jewish bankers; and criticized the policies of Léon Levy, the Jewish director of a large public corporation that owned mines and factories throughout the region. *Le tocsin*, 1891–1895; *Le tocsin populaire*, 1897–1898.

¹³³ Eugen Weber, "Jews, Antisemitism, and the Origins of the Holocaust," *Historical Reflections*, 5 (1978): 6–9; Irvine, *Boulanger Affair*, 170.



FIGURE 8: This image appeared in *Le grélot* on November 11, 1894. It was similar to many other cartoons in a variety of popular newspapers throughout the Dreyfus Affair. Here, socialists (Jules Guesde and Jean Jaurès) are linked with Jewish financiers. All were “sans-patrie” or without a country. Courtesy of the Houghton Library, Harvard University.

large landowners.¹³⁴ Moreover, as early as 1895, the neo-Blanquist Thivrier warned of the danger of defining the Jewish problem as a question of race. He insisted that capitalism, not Judaism, created bankers and that the same problem would exist without Jews. He also reminded readers that most of their exploiters were not Jewish. “Is it necessary under these circumstances,” he asked, “to accuse the Jews?”¹³⁵

A few years later, the Guesdist Constans also tried to explain his position on property and Jews to peasants in the viticultural village of Domérat. He rhetorically asked them, “Where does the right to private property come from?” “Not from the Bible,” he answered. “Religions begin with moral principles, not defense

¹³⁴ *La socialiste de l'Allier*, January 30, 1898, February 27, 1898; *La croix de l'Allier*, January 30, 1898. See also Green, “Changing Attitudes of French Socialists.”

¹³⁵ *Le tocsin*, June 1, 1895.

of private property . . . but now all religions defend the rights and privileges of the rich." To loud applause, Constans concluded, "There are no longer Christians, Jews, Muslims, Monarchists, or Republicans. You will only find a coalition of capitalists. It is against this coalition that socialists must struggle."¹³⁶

Both socialists argued that voters had to learn to struggle against capitalists as capitalists, not as Jews. Dramatically rejecting the imaginary community of the right that demanded a "France for the French," they argued that all such artificial divisions simply confused the basic issue of exploitation. As the neo-Blanquist *Le tocsin populaire* explained just before the elections, what the Jesuits and their financier friends meant by the slogan "'France for the French' was 'France for us and us alone.' You should not be duped by it."¹³⁷ The Republic, "imperfect as it is," Constans insisted, "must serve as our instrument of emancipation."¹³⁸

Constans' position is important, for it reflects the impact of the Dreyfus Affair on some socialists in France. Before 1898, socialists of both Allier factions participated in elections, but they were ambivalent toward parliamentary politics. The Dreyfus Affair, by threatening the Republic with another coup d'état that would put in power a strong leader with popular support based on anti-Semitism, induced many socialists to defend republican institutions. Eventually, the specter of popular right-wing, anti-Semitic nationalism in the countryside turned both of the Allier socialist factions toward parliamentary politics, in spite of their official affiliations with the anti-parliamentarian Guesdists and neo-Blanquists.¹³⁹ Ironically, socialism—a movement that in its *fin-de-siècle* form stood as a program to overthrow bourgeois parliamentarianism—ended up in rural areas as one of parliament's strongest supporters, although the socialists reserved the right to disagree. For many early twentieth-century intellectuals, the extreme left's reluctant turn toward social democracy represented the crucial culmination, if not the failure, of the Dreyfusard Revolution.¹⁴⁰

IN ANALYZING THE SIGNIFICANCE OF THE DREYFUS AFFAIR in much of rural France, one must determine what commonalities characterized those areas that enthusiastically embraced anti-Semitism and the new nationalism based on race. It seems that anti-Semitism became important in some departments where traditional conservatism, based on the politics of personalities, was in the process of collapsing but could still pose a threat to its enemies. In these departments, anti-Semites appealed to anti-government and strong anti-socialist sentiments. The national and even local press itself became a new site for their politics.

By the end of the nineteenth century, many local notables no longer unproblematically controlled their rural constituencies and longed to restore the old

¹³⁶ *La socialiste de l'Allier*, July 4, 1897; see also May 19, 1895.

¹³⁷ Camille Lejuste, "L'antisémitisme," *Le tocsin populaire*, April 25, 1898.

¹³⁸ *La socialiste de l'Allier*, July 4, 1897.

¹³⁹ See especially *Le tocsin populaire*, January–December 1898.

¹⁴⁰ This was particularly true of Georges Sorel and Gustave Hervé. See Robert Louzon, "La faillite du Dreyfusisme ou le triomphe du Parti juif," *Le mouvement socialiste*, no. 176 (July 1906): 193–99; Sternhell, *Le droit révolutionnaire*, 27–31; Georges Sorel, *La révolution dreyfusienne*, 2d edn. (Paris, 1933).

order wherein they dominated politics without competition. Even their 1898 campaigns reflected their attitudes toward parliamentary politics. Gers deputy Cassagnac, like many conservatives throughout France, belligerently campaigned without a platform, claiming that voters should vote for him because they had voted for his father.¹⁴¹ Marcel Vacher, a large landowner who lost his bid for reelection in the Allier, held one large private banquet for his constituents at which he insisted that there was no class struggle because no classes existed, adding that what socialists called class struggle was in reality only a conflict between personalities.¹⁴² Louis-Eugène Bignon, another Allier notable who lost in the 1898 election, did not campaign at all but visited villages with the local subprefect to suggest that people should vote for him because he was the official candidate.¹⁴³ Other candidates, especially from western France, campaigned as *enfants du pays*, reminding voters that everyone knew them, that everyone knew their fathers and grandfathers, that everyone knew they possessed titles of nobility.¹⁴⁴ Their activity contrasts sharply with the many popular electoral rallies sponsored by victorious Radicals and socialists, who often could make no such personal claims and campaigned on issues.¹⁴⁵

As William Irvine has demonstrated, there was nothing natural about the conservatives' political transformation. Previously, conservative rural politicians often depended on Jewish money for their campaigns, and many were married to Jewish women. Many of them found distasteful the individuals and street politics associated with anti-Semitic leagues. In 1889, Cassagnac had published an article ridiculing anti-Semitism.¹⁴⁶ As late as the early 1890s, the pretender, the count of Paris, had specifically rejected any suggestions that Royalists consort with anti-Semites like Drumont. Moreover, although some Royalist, Bonapartist, and clerical groups employed anti-Semitism in 1893 elections, they were largely unsuccessful because "anti-Semitism had not yet penetrated to the popular level, to the mass of voters."¹⁴⁷

Mass culture did have an impact on the countryside, then, as it was appropriated by some agrarian conservatives, who turned toward anti-Semitism and a redefined nationalism when they were compelled through the process of democracy to compete for votes with left-leaning Radicals and socialists threatening the basis of their existence. The press, the special commemorative objects, and other instruments of mass culture and commercialization invaded the provinces with constant information and opinion about the Dreyfus Affair and the "Jewish menace." They helped create an imaginary community among political groups that otherwise had little in common. Joined through an amorphous ideology grounded in concrete local concerns, anti-republican conservatives, conservative

¹⁴¹ France, *Programmes*, 1898, 278–80.

¹⁴² Both Bignon (see below) and Vacher became active nationalists who distributed anti-Semitic literature in their electoral campaigns. *La socialiste de l'Allier*, January 16, 1898; ADA 3M 61–65.

¹⁴³ *La socialiste de l'Allier*, February 27, 1898.

¹⁴⁴ France, *Programmes*, 1898, 48, 126–27, 133–35, 149–50, 162–66, 186–87, 207, 219–20, 307–08, 312–15, 379–81, 414–15, 469, 754–55.

¹⁴⁵ Bordes, "L'évolution politique," 22; *La socialiste de l'Allier*, 1895–1902.

¹⁴⁶ *L'autorité*, October 26, 1889, as cited by Irvine, *Boulangier Affair*, 167.

¹⁴⁷ Raphael Viau, *Vingt ans d'antisémitisme* (Paris, 1910), 60. See also Irvine, *Boulangier Affair*, 157–76.

republicans, and many moderate Radicals developed similar perspectives on economics, defense of private property, and opposition to state socialism.¹⁴⁸ Most important, landed notables began to articulate the view that the parliament as an institution was very “foreign” and not French.

The Dreyfus Affair thus entered rural villages within a particular context, which enhanced its significance by joining anti-Semitism to a movement to challenge the legitimacy of the democratic political institutions developed after the French Revolution. Contradictory ideas on the form parliamentary government should take began to emerge in France before the revolution, before either the full institutionalization of universal manhood suffrage or the rise of a mass-based commercialized press. They combined both feudal and revolutionary ideas of government, and those that triumphed tended to create a system of representation grounded in patronage and deference. Beyond doubt, the difficulty of carrying out principled foreign and domestic policy under the pressure of mass democracy and an aggressive press had not been fully envisioned. For *fin-de-siècle* agrarian notables, parliamentary politics, which might lead to Radical and socialist victories, was completely out of control. For them, the twentieth century demanded new political processes rooted in a movement that could appeal to passions while promoting authoritarian rule. The rhetoric associated with the Dreyfus Affair collectively promoted just such a politics. From the beginning, conservative anti-Semites aimed to appropriate certain language from the French Revolution to articulate a “right-wing” Jacobinism. Willing to accept a limited notion of universal suffrage, they rejected “parliamentarianism, committees, parties . . . [as well as] the entire philosophical and ethical complex that allowed the Jew to be regarded as a full-fledged member of the French collectivity.”¹⁴⁹

A confluence of specific cultural, political, and socioeconomic changes transformed some very traditional ideas into a virulent form of twentieth-century anti-Semitism, which became the basis of a new nationalism best epitomized in the slogan, “France for the French.” It seems that the emergence of this new nationalism in the countryside developed out of the near total collapse of traditional conservative politics in *fin-de-siècle* France, as politics based on local social networks began to give way to mass-based, nationally focused political parties, particularly those organized to struggle for working-class interests. Desperate for an alternative to appeal to, the anti-Semitic nationalists reworked elements of traditional popular thought to construct the modern scapegoat of the Jew, the Jew designed to stand in for the worst aspects of capitalism, the Jew who could be identified as the source of individual economic hardship, the Jew who could be simultaneously foreigner, international financier, anarchist, and socialist.

¹⁴⁸ Offen, “Paul de Cassagnac,” 236–37, has an excellent analysis of this point.

¹⁴⁹ The quotation is from Sternhell, who wrongly believed that the new right also rejected universal suffrage, hence Jacobinism in its entirety. In fact, it was quite selective in terms of its appropriation. Sternhell, “Roots of Popular Anti-Semitism,” 133–34. See also Jean-Paul Sartre, *Anti-Semite and Jew*, George J. Becker, trans. (New York, 1948), 55–144; Stanley Diamond, *In Search of the Primitive: A Critique of Civilization* (New Brunswick, N.J., 1974), 324–31; and Weber, “Jews, Antisemitism, and the Origins of the Holocaust,” 1–17.

Dreyfus as the Jew/traitor could signify everything because in himself he signified nothing. In Dreyfus's own words, "Dreyfus the symbol . . . is not me."¹⁵⁰

There was nothing inevitable about the formation of this new nationalism grounded in anti-Semitism. It was an ideology in search of a popular following. But the *fin-de-siècle* economic crisis combined with anti-Semitic and anti-Dreyfusard images spread in newspapers and articles of consumption helped create a powerful and popular message for the forces of the right.

If the effect on the rural right was to make anti-Semitism and anti-parliamentarian nationalism its central concerns, the effect on the rural left was most visible in the elections held in the wake of the Affair in places like the Allier. In response to the right's new programs, many socialists came to defend Dreyfus and the Jews (at least as poor workers), while abandoning their extreme hostility to parliamentary institutions. Consequently, they tempered other party lines, in particular their position on the collectivization of property. Like the right, they too wanted to win elections. Because of the socialist response, after the Dreyfus Affair not only did parliamentary socialism become most important in predominantly rural departments but its largest successes came in the most agricultural regions of these departments.¹⁵¹

In retrospect, the Affair's most enduring impact may have been its ability to reformulate traditional rural alliances and rhetoric to establish new political forces. Many rural departments like the Allier continued to provide the social base for left-wing parties into modern times. Moreover, while we should avoid overemphasizing the significance of the nationalist electoral victories in 1898 as well as the simplistic similarities between the coalition forged in 1898 and subsequent right-wing politics, it could be argued that the tendencies embodied in the coalition never entirely disappeared. Although authoritarian republicanism has become increasingly secular and urban, one can point to rural and religious influences in many of its twentieth-century forms.¹⁵²

To understand how a new ideology, even when constructed out of reconfigurations of traditional images and ideas, develops widespread appeal, we must examine precisely how it is insinuated into a variety of local circumstances—both urban and rural. Ideologies that cannot be merged with local concerns are unlikely to develop impressive followings. It is because the Jew as an abstraction could be attached to economic dislocation in the Gers that five out of five deputies elected were anti-Semitic. In the Allier, where organized socialists successfully challenged the rhetoric of the anti-Semites as an obfuscating discourse, it was much more difficult for the anti-Semitic right to gain a foothold. Here, where socialists recognized that anti-parliamentarianism would ultimately benefit the right more than the left, they launched a campaign to defend the Republic at all costs. In the words of the socialist sharecropper leader from the Allier, Emile

¹⁵⁰ As cited by Wilson, *Ideology and Experience*, 1.

¹⁵¹ Judt, *Socialism in Provence*, 296–305; *La socialiste de l'Allier*, April 3, 1898, June 5, 1898, June 25, 1899.

¹⁵² Offen, "Paul de Cassagnac," 403.

Guillaumin, "Despite its vices and its shortcomings, one has to admit that the composition of parliament affects our daily life."¹⁵³

How the anti-Semitic and nationalist ideas associated with the Dreyfus Affair were acted on or rejected thus depended on a complex set of local socioeconomic and political factors. Not everyone accepted them. They did not resonate similarly everywhere. However, they flourished in certain parts of rural France, where anti-Semites seem to have accepted Drumont's assumption that ordinary people were "unable to coordinate all of these ideas" and "newspapers had to assume the responsibility of explaining" the danger of cosmopolitan Jewry.¹⁵⁴ In these rural areas were politicians who believed, as did the fictional character Woldsmuth in Roger Martin du Gard's *Jean Barois*, that "one can do what one likes with a people, when one knows how to excite them against Jews."¹⁵⁵

¹⁵³ *Le travailleur rural*, September 1910.

¹⁵⁴ Edouard Drumont, as cited in Wilson, *Ideology and Experience*, 606. See also *La croix nantaise*, February 6, 1898; and "Rapport du commissaire central de la réunion du 9 novembre 1898," as cited in Fabrice Abbad, ed., *La Loire-Atlantique des origines à nos jours* (Saint-Jean-d'Angély, 1984), 358–59.

¹⁵⁵ Roger Martin du Gard's novel set in the midst of the Dreyfus Affair first appeared in 1913. See *Jean Barois* (Paris), 290.

Anarchism, Political Participation, and Illiteracy in Barcelona between 1934 and 1936

MERCEDES VILANOVA

SPAIN IN THE YEARS 1934 to 1936 was the most important center of anarchist thought in the world. Spain had no tradition of compromise within a framework of democratic institutions. At the same time, action in left-wing trade unions and in political parties at the extremes of both right and left had an exalting moral dimension. In 1869, Mikhail Bakunin's representative Giuseppe Fanelli had visited Spain; two years later, Spanish anarchists could claim almost 100,000 members, something not achieved in the same way in any other country. The influence of the Anarcho-Syndicalist trade union CNT (National Confederation of Labor), founded in Barcelona in 1911, was very strong, especially during the 1930s, when its membership grew by the hundreds of thousands. Spanish anarchists were known for their stress on the importance of education. Stories passed down by writers from other nations who came to observe and to participate in the making of history show that literacy for anarchists was a key to autonomy and empowerment. In Frank Jellinek's description, "Workers who could just read [and not write] made the most fantastic sacrifices to buy Elisée Reclus's . . . *History of Mankind* and a pistol. With both they could conquer the world." Recently, comparisons have been made to Spain in 1930 to England in the 1840s or France in the 1860s. Of course, the working class of Barcelona in the 1930s cannot be easily compared to the English working class of the nineteenth century, but their respective levels of literacy and illiteracy were without doubt decisive factors in union organizing.¹

It is well known that anarchists counseled their adherents to abstain from the municipal elections and usually from all elections. Abstention was an act not of withdrawal from politics but of calculated participation in it, designed to delegitimize a political system and show its moral bankruptcy. In this sense, for the anarchists, abstention was a political act of the highest order. Of all the elections

I have been able to write this article thanks to a fellowship from the Woodrow Wilson International Center for Scholars during the year August 1989 to August 1990. My thanks to Cristina Boix for letting me use the data on electoral trajectories of Barcelona between 1934 and 1936, and to Anna Monjo and Carme Vega for the data from a metallurgical factory between 1934 and 1940.

¹ E. P. Thompson insisted on the close links between the diffusion of literacy and the organization of workers' protest as one of the subsidiary themes of *The Making of the English Working-Class* (New York, 1963). I am grateful for the comments and written answer of Professor Daniel P. Resnick of Carnegie Mellon University to my paper, "Political Participation and Illiteracy: Barcelona during the 1930's," presented at the Wilson Center on July 31, 1990. I have taken some of his ideas to write this brief introduction.

held between April 1931—the beginning of the Second Republic—and July 1936, when General Francisco Franco made the military coup that started the Spanish Civil War, the most historically decisive was that of February 1936, known as the Popular Front election. Hope for the release of the persons imprisoned in connection with the Asturian Revolution of October 1934 is usually given as a major reason why anarchists voted in February 1936 in considerable numbers. Historians have seen the electoral victory of the Popular Front² as the immediate cause for an outburst of social unrest and subsequent outbreak of the civil war, which ended with Franco's victory three years later. Literacy and illiteracy played important roles in the voting behavior that led up to these decisive events. This article explores this impact and especially the personal implications of illiteracy for a small sample of survivors of the Spanish Civil War.

THE ANARCHO-SYNDICALISTS AND THEIR SLOGAN "Do Not Vote" have been considered to have had an extremely important influence on the political fluctuations of the Second Republic.³ To many historians, the anarchists were so powerful that, when workers acted on the slogan "Do Not Vote" in 1933, the right was able to win the general elections. When voters did not follow the slogan in 1931 or in the Popular Front election of 1936, the left won. This assumed pattern of anarchist electoral behavior has been an easy way to explain the changes in the Spanish parliament during the 1930s, and this myth of the anarchist abstention has persisted because it served several purposes. It magnified the strength of the anarchists, while the Marxist left could rationalize the low level of Communist votes by pointing to the abstention of great majorities of workers. Finally, the supposed power of the anarchists provided an excuse for the extreme right to abolish democratic institutions on the grounds that anarchist workers were uncivilized and unable to understand the function of political consensus and parliamentary government.

This study covers the last three elections held in Barcelona during the Second Republic prior to the civil war: the local *Municipales* of 1934; the legislative election for the Spanish parliament, the Popular Front election of February 1936; and the *Compromisarios* of April 1936, an election to choose delegates who, together with the parliamentary deputies, would then choose the president of the Republic.⁴ The *Compromisarios* election was the last to take place during the Second Republic. The outcome of the approaching presidential election was

² The Popular Front received 47.6 percent of the vote, the center was much weakened with only 9.3 percent, and the right received 43.5 percent. The right was strongest in the two Castilles and Navarre, and the Popular Front in Catalonia, the Basque provinces of Asturias, Galicia, and Andalusia.

³ A very revealing work, printed many times, that has contributed to the generalization of this myth is Pierre Vilar, *Histoire de l'Espagne* (Paris, 1947), 109–10, translated into English by Brian Tate as *Spain, a Brief History* (Oxford, 1967, 1977).

⁴ In the *Municipales* elections, the Left Front received 50.33 percent of the vote, the Lliga or Party of the Catalan Bourgeoisie 41.35 percent and other minor parties a much smaller percentage. In the Popular Front elections, the Popular Front obtained 62.66 percent of the vote, and the Order Front or the right got 37.34 percent. In the *Compromisarios* elections, the Popular Front received 75.16 percent of the vote and the Lliga 24.84 percent.

widely known beforehand, and the right therefore abstained from the *Compromisarios* almost everywhere in Spain, except in Catalonia. There was little public interest in this election, and Barcelona showed the lowest level of participation (34 percent) of all the general elections held during the Second Republic.

Our research group studied patterns of electoral behavior for these three elections through a stratified sample of the Barcelona electorate.⁵ Taken from a census of about 600,000 electors for the last two elections of 1936, this data set contains 41 electoral sections for a total of 24,798 electors, of which 4,510 are illiterates. For the municipal election of 1934, we sampled 12 electoral sections with a total of 7,034 electors, of which 1,154 are illiterate.⁶ By tracking individuals across data sets, we were able to uncover the relationship between voting patterns over time and other variables like gender, age, wealth, party membership, literacy, and position in the work force. In addition to analyzing each election separately, we took into account the eight possible "electoral trajectories" resulting from different combinations of participation or abstention in the three elections.⁷ For example, we were able to determine that a specific individual voted in the municipal election, abstained in the Popular Front election, and voted again in the *Compromisarios*. This track record of voting forms an individual's electoral trajectory. The literature on electoral trajectories is not abundant because in all other countries, voters' records are typically destroyed within days of the election. Republican Spain is an exception; even today, nearly complete vote counts are preserved in the archives of the county councils (*diputaciones provinciales*). We know of no other studies that analyze the electoral trajectories of illiterates.⁸

Our analysis revealed that ideological abstention did not influence the result of the elections.⁹ We also discerned an inferior participation by women in Barcelona that roughly coincided with those of other countries during the 1930s (Table 1).¹⁰ This lesser participation by women can be generalized to the whole region, as I

⁵ Cristina Boix Serra, *El abstencionismo electoral durante la Segunda República (1934–1936): Un programa para su estudio* (Tesis de Licenciatura, Departamento de Historia Contemporánea, Universidad de Barcelona, 1979). I have used this same sample in this study.

⁶ The election of 1934 has only been studied with 7,034 electors and not the total sum of 24,798 electors because I have been unable to find the required data for 1934; these data were probably burnt during the war.

⁷ Since in each election, there are two possible options—to vote or not to vote—in n elections, there will be 2^n possible combinations of participation and abstention, in our case, 2^3 ($2 \times 2 \times 2 = 8$), and each one of these eight possible combinations is called an electoral trajectory.

⁸ In the USA, the only two studies we know of that analyze illiteracy and political participation at an individual level are by Charles Edward Merriam, *Non-Voting: Causes and Methods of Control* (Chicago, 1924); and Harold F. Gosnell, *Getting out the Vote: An Experiment in the Stimulation of Voting* (Chicago, 1927). No doubt the difficulty in identifying illiterate electors is the reason why there are practically no studies about political participation and illiteracy.

⁹ Mercè Vilanova, *Atlas electoral de Catalunya durant la Segona República: Orientació del vot, participació i abstenció* (Barcelona, 1986), see especially chap. 4, "El comportament de l'electorat català durant la Segona República."

¹⁰ According to Herbert Tingsten, *Political Behavior: Studies in Election Statistics* (London, 1937), 229, "It has been shown that the women nowhere make use of their vote to the same extent as the men; as a rule the difference in voting frequency between the sexes in recent years seems to have amounted to about ten per cent." Also see Mattei Dogan and Jacques Narbonne, "L'abstentionnisme électoral en France," *Revue française de science politique*, 4, nos. 1–2 (January–February and April–June 1954): 25: "Il existe généralement, un certain parallélisme entre l'abstentionnisme masculin et féminin: ou le premier est faible, le second l'est aussi, et vice versa. L'écart entre le moyenne de l'abstentionnisme masculin et féminin ne varie que de 6 percent à 12 percent au maximum."

TABLE 1
Participation of the Electorate of Barcelona between 1934 and 1936, in the Entire City and in the Samples; Globally and by Sex, in Percentages

	CITY <i>Global Participation</i>	SAMPLES		
		Men	Participation Women	Both
January 14, 1934	53	57	45	50
February 16, 1936	65	67	58	64
April 26, 1936	34	41	27	33

learned while conducting similar studies in a number of villages of Catalonia. In the larger villages or cities, however, women tended to vote more frequently than they did in the small villages, and therefore the coefficient of correlation between female voting and electoral census or size of villages is slightly positive (.2).¹¹ After these initial studies, we decided to examine the role of illiteracy in abstention, for the high levels of abstention seemed to be at odds with the significance of the majority of the republican elections.¹² It was then we discovered that the differential of participation of men and women could be further refined by taking literacy into account.

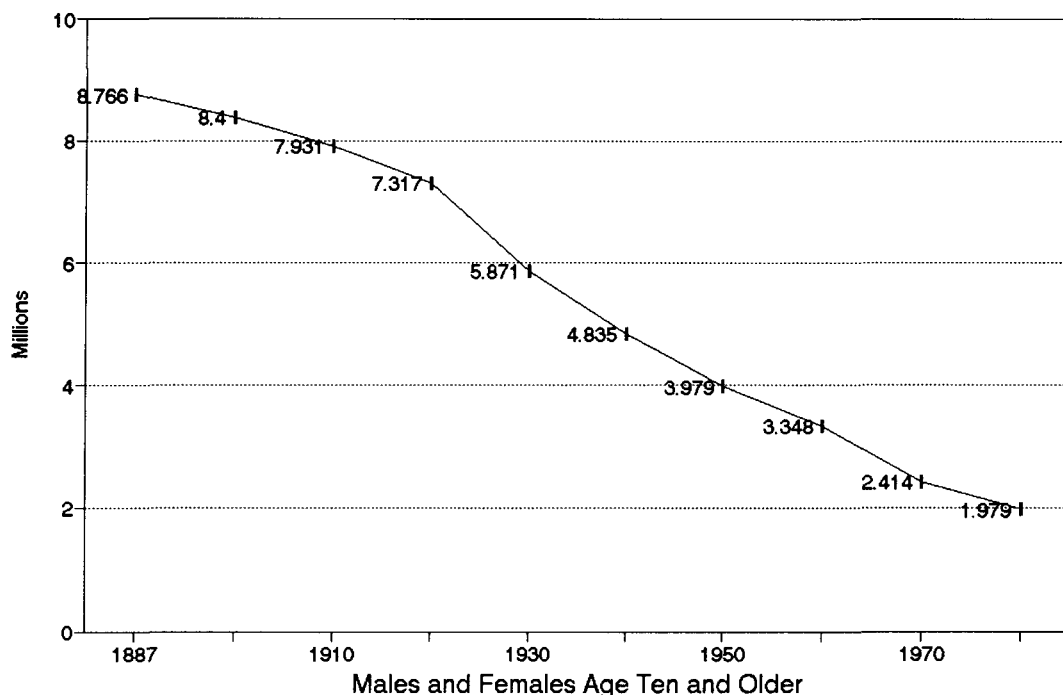
In the history of modern Spain, illiterates have not been given the attention that they merit in view of their large numbers and distinguishing characteristics (Figure 1).¹³ During the 1930s, the illiterate population of ten or more years of age, as determined by the census, represented 32 percent of the total population, with an even higher percentage among females (Figure 2). Barcelona at this time was a largely working-class city with levels of illiteracy less than the average for Spain as a whole. More than two-thirds of the men were classified as skilled

¹¹ The matrix of the electoral coefficient of correlation for Catalonia during the Second Republic is found in Vilanova, *Atlas electoral de Catalunya*.

¹² Harold F. Gosnell, *Why Europe Votes* (Chicago, 1930), 19, considers that the index of participation is a function of the valid alternatives that are presented to the electorate. Following this criterion, the Popular Front election would be a contested election, while the *Compromisarios* would clearly not be. The *Municipales* of 1934, keeping in mind the triumph of the right in 1933, would also be a contested election, because in 1934, according to Molas, the system of blocs was attempted in the city of Barcelona. Molas, *Lliga Catalana: Un estudi d'estasiologia*, 2 vols. (Barcelona, 1972).

¹³ A classic work on this topic is that of Lorenzo Luzuriaga, *El analfabetismo en España*, 2d edn. (Madrid, 1926). Another more recent work, interesting for the facts it contains, is A. Guzmán Reina, F. Rodríguez Garrido, and A. Cerrolaza Asenjo, *Causas y remedios del analfabetismo en España* (Madrid, 1955). A doctoral thesis on the education of schoolteachers but with an update of data on illiteracy and, above all, on the quality of education in Spain is Julia Melcón Beltrán, *La enseñanza elemental y la formación del profesorado en los orígenes de la España contemporánea: Renovación pedagógica y enseñanza de la geografía* (Universidad de Barcelona, 1988). In 1930, the Spanish school system was deficient from every point of view; it was 17,500 schools short, the teachers were poorly prepared and even more poorly paid, the classrooms were packed with students, and those who nevertheless learned to read also lived at such a low economic level that they could barely afford to buy books. See J. F. Botrel, "L'aptitude à communiquer: Alphabétisation et scolarisation en Espagne de 1860 à 1920," *Communication to the colloquium, Instruction, lecture et écriture en Espagne (XVI^{me}-XIX^{me} siècles)* (Toulouse, 1982). No studies of Spain during the Republic, the civil war, or the Francoist period qualitatively analyze illiteracy. We made an initial attempt at this in Dominique Willems and Mercedes Vilanova, "Lengua y poder en Cataluña durante los años treinta," in Mercedes Vilanova, ed., *El poder en la sociedad* (Barcelona, 1986). For a quantitative perspective, see Mercedes Vilanova and Xavier Moreno, *Evolución del analfabetismo en España de 1887 a 1981* (Madrid, 1992).

Figure 1
Illiterate Population of Spain
1887-1981



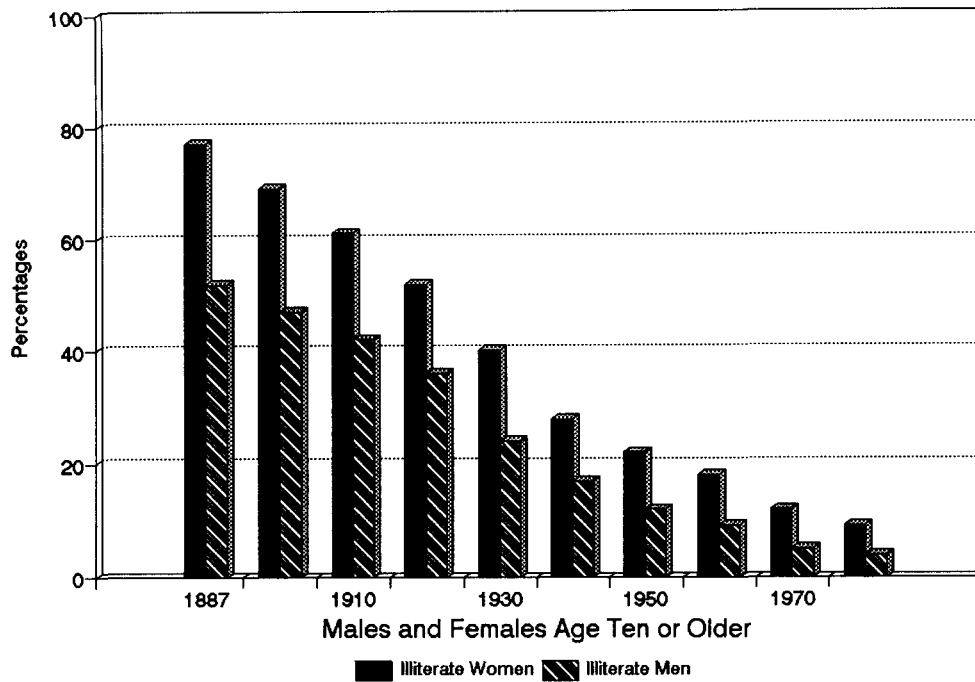
Source: Censuses of Spanish Population

laborers, unskilled laborers, or day laborers. The majority of women worked as maids, day laborers, or simply housewives (Tables 2 and 3). As in other parts of Spain, higher percentages of women than men were illiterate in Barcelona, roughly two-and-a-half times higher. Ten percent of the men were illiterate and 25 percent of the women, although in certain parts of the city, illiteracy reached more than 50 percent among women and 25 percent among men (Tables 4 and 5). If we take into account the different jobs held by women, then *concierges*, day laborers, specialized workers, and housewives had the highest illiteracy (Table 6). For males, illiteracy was greatest among fishermen, sailors, textile workers, doormen, and day laborers (Table 7).

Examining gender, literacy, and age together in each election, we could see that non-voting followed the same general pattern in the last three elections of the Republic, but the base level of non-participation differed. During the Popular Front elections, the base level of non-voting was quite low. It was higher during the *Municipales* and higher still during the *Compromisarios* (Figures 3 and 4). The key factors in non-voting were literacy and gender, except during the *Compromisarios* election (Figure 5). Whereas within the illiterate population, it was women who abstained most often, among the literate, women were more politically active.

Figure 2

Male & Female Illiterates Spain 1887-1981 (in percentages)



Source: Censuses of Spanish Population

The electoral trajectories demonstrate that almost 50 percent of illiterate women and more than 30 percent of illiterate men never voted, an impressive example of their political marginality (Figures 6 and 7).

On the other hand, the *Compromisarios* election constitutes an interesting exception in which illiterate men tended to vote more in all areas. In all the villages studied in Catalonia (Beuda, La Escala, and Sant Feliu de Guixols), illiterates voted in the *Compromisarios* election in higher percentages than in earlier elections. The electoral behavior in the city of Barcelona showed a similar result. Thus a characteristic of the male illiterate population of Catalonia during the 1930s was that they voted more when the literate population tended to vote less. The relatively high level of participation by illiterates during the last democratic election before the Franco dictatorship indicates that the illiterates conducted an intense electoral battle in this period of extraordinary social conflict, just three months prior to the outbreak of civil war. I know of no other comparable example. The relatively high participation of illiterate men, above all, the young men, in an election whose results were known in advance is remarkable and clearly differentiates illiterate men from the rest of the electorate (Figure 3).

The electoral trajectories reveal the behavior of illiterate males more graphically. In Barcelona, illiterate men tended to follow a specific trajectory (parti-

TABLE 2
Distribution of Professions of Women, by Percentage, Sample from the 1934
Electoral Census of the City of Barcelona

<i>Profession</i>	<i>Women (%)</i>
Housewife*	80.0
Day Laborer	6.0
Maid	5.5
Shopkeeper	0.7
Concierge	0.6
Religious	0.6
Worker	0.5
Other	6.6

*The most symptomatic information gap in the electoral census is the silencing of working women. "Her own work" (which we have translated as "housewife") was noted as the profession the majority of the time, but we have been able to corroborate that many of these women worked in factories or at home doing piecework.

pation in the Popular Front and *Compromisarios* but abstention during the *Municipales*, five percentage points more often than literate men (Figures 7 and 10). When this trajectory, which I will call "singular," is considered by age group, the absolute numbers of the sample are much reduced; nevertheless, the difference in percentages of literate and illiterate men between twenty-one and twenty-five years of age is certainly disproportionate. Of the illiterates of this age group, 67 percent fall into the singular trajectory, while only 8 percent of the literates can be classified in the same way. This is a clear indication that it was young male illiterates who tended to participate most during the *Compromisarios* election. The importance of participation by young illiterates is also observed in the analysis of behavior by election, age, and degree of literacy (see Figures 4 and 5). The literate population tended to follow with greater intensity the electoral trajectories that included participation in the *Municipales* and Popular Front elections, in which more than one candidate had a chance to win. The fact that the illiterate population tended not to participate during the *Municipales* but did participate in the *Compromisarios* can be interpreted in a variety of ways. María

TABLE 3
Distribution of Professions of Men, by Percentage, Sample from the 1934
Electoral Census of the City of Barcelona

<i>Profession</i>	<i>Men (%)</i>
Day Laborer	51.5
Specialized Worker	12.0
Shopkeeper	9.5
Employee	6.8
Liberal Professional	2.5
Student	1.0
Retiree	1.0
Serviceman	0.6
Other	15.1

TABLE 4
Illiterate Population, 10 or More Years of Age, and the Distribution by Sex, in Percentages, According to the Population Census of 1930

	<i>Illiterate Population</i>	<i>Percent of the global population (age 10 or older)</i>	<i>Males (%)</i>	<i>Females (%)</i>
Spain	5,871,403	32	36	64
Catalonia	490,155	21	34	66
Barcelona City	127,580	15	26	74

Goñi, who has studied six elections in the town of Sant Feliu de Guixols during the Second Republic, concluded that the illiterates participated one election “behind” everyone else. In this way, they acted as a “brake,” smoothing out the curves of abstention and participation. An analysis of this singular trajectory in a Barcelona working-class neighborhood such as Pueblo Nuevo, which had high levels of illiteracy, shows the percentage of illiterate men who follow it to be more than double the rest of illiterate men of Barcelona (Figures 7 and 8). We wanted to know why illiterate working-class men followed this trajectory so exclusively.

To reach a better understanding of the workers’ behavior, we decided to study them in the workplace. A metallurgical firm with approximately 1,600 workers was chosen.¹⁴ From the company’s documents, we were able to determine if the workers were blue collar or white collar, moderate or radical, affiliates, militants, or leaders.¹⁵ Unfortunately, this documentation did not include data on illiteracy; these we had to find in the electoral sources. The firm’s documents, together with the electoral census, allowed us to create a list of voters that included ideological affiliation and level of literacy as well as to determine the electoral trajectory of each worker. Because the electoral census of Barcelona during the 1930s

TABLE 5
Concentration of Illiteracy in the Electorate of Pueblo Nuevo, by Percentage, An Example, District 10, Section 32, with 589 Electors, Electoral Census of the City of Barcelona in 1934

Literate men	74%	Illiterate men	26%	Total	100%
Literate women	48%	Illiterate women	52%	Total	100%
Literate electorate	58%	Illiterate electorate	42%	Total	100%

¹⁴ This metallurgical firm is the Riviere factory, “collectivized” during the civil war under the name Trefilerías Barcelonesas. Its “collectivization” has been studied by Anna Monjo and Carme Vega, *Els treballadors i la Guerra Civil: Història d’una indústria catalana collectivitzada* (Barcelona, 1986).

¹⁵ We defined as “moderate” those who accepted the Popular Front pact and the collaboration between different groups of workers in the factories during the civil war; as “radicals” those who did not accept the pact and went over, during the civil war, to the syndical opposition in the factories. “Blue-collar” workers are all production workers regardless of skill level. “White-collar” workers are “office” workers in the administration of the factories. Given that union affiliation was mandatory, we considered as “affiliated” all of the workers in the factory and “militants” as those who, during the civil war, participated in committees or commissions of study or spoke publicly in the general assemblies and those who were purged or exiled after the war, even if their support for “collectivization” was not documented.

TABLE 6
Distribution of Illiteracy among Women by Professions, in Percentages

<i>Profession</i>	<i>Illiteracy (%)</i>
Concierge	37
Day Laborer	32
Worker	27
Housewife	27
Maid	21

contained more than 600,000 electors grouped by domicile of electoral precinct but not in alphabetical order, and documentation from the company did not contain the addresses of the workers, we were able to find only about 300 electoral trajectories of the workers in question. We are not therefore dealing with a statistically significant sample, but it is nevertheless a sample about which we know a great deal through the written and numerical materials from the firm and through interviews with a total of eighty-two survivors.¹⁶

After reconstructing 300 electoral trajectories of workers during the three elections from 1934 to 1936, we projected them onto a schedule of the workers' personal socio-political behavior during the civil war and the subsequent Francoist repression.¹⁷ This analysis made clear that the repression did not punish the illiterate population with the same vigor as the literate population, except for the illiterate women who had militant fathers, husbands, or brothers and were fired because of their family relationships. We also discovered that for each trajectory there was a corresponding level of leadership or militancy and even a defined level of Francoist repression. The leadership of the white-collar workers always voted, and only after the war did they lose their jobs; the moderate anarchists who voted only in the Popular Front elections suffered a more benign purge; but a minority of the anarchist leadership in the CNT trade union were radical and ideological nonvoters, and after the war they suffered execution, or torture and long years of jail. For example, one of our interviewees was a factory leader who

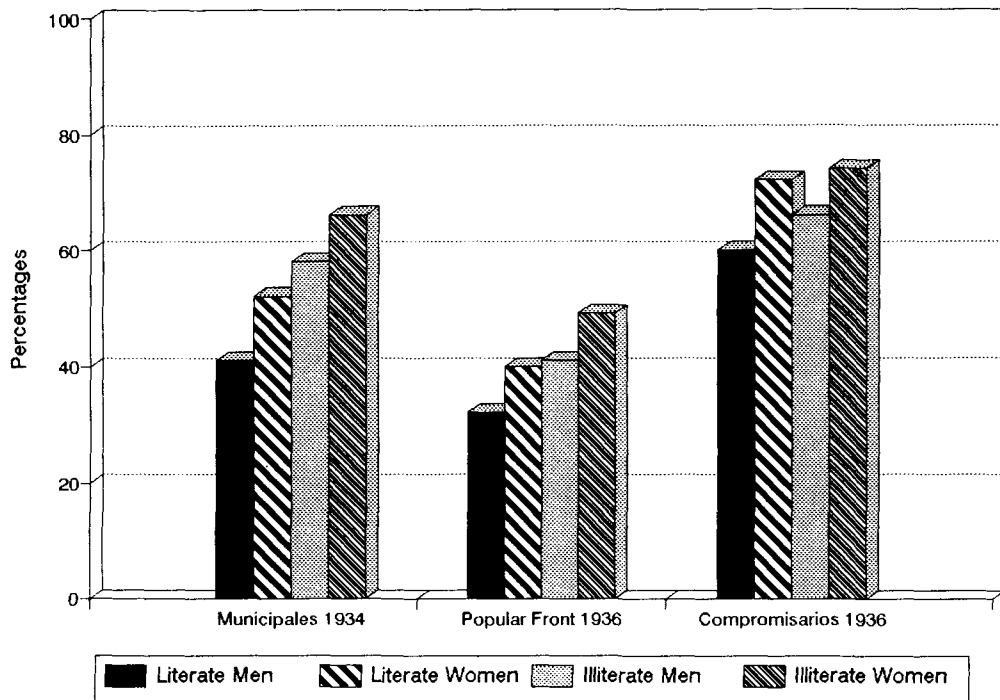
TABLE 7
Distribution of Illiteracy among Men by Professions, in Percentages

<i>Profession</i>	<i>Illiteracy (%)</i>
Fisherman	60
Sailor	41
Doorman	23
Worker	20
Day Laborer	18

¹⁶ We were able to interview seventy-six people; of these, we knew the trajectories of fifty-one. To these seventy-six must be added six who were interviewed in Mijas in order to have a point of comparison with rural illiteracy. My thanks to Ronald Fraser, who provided us with the transcription of the interviews he conducted for his book, *Tajos: A Spanish Village in the Costa del Sol* (London, 1973); I am also grateful to him for providing the list of informants and their addresses.

¹⁷ A. Monjo, C. Vega, and M. Vilanova, "Trajectoires electorales, leaders et masses sous la II^{ème} République en Catalogne," *Il politico*, 48 (1983).

Figure 3
Non Voting by Literacy & Sex
in Barcelona during Three Elections



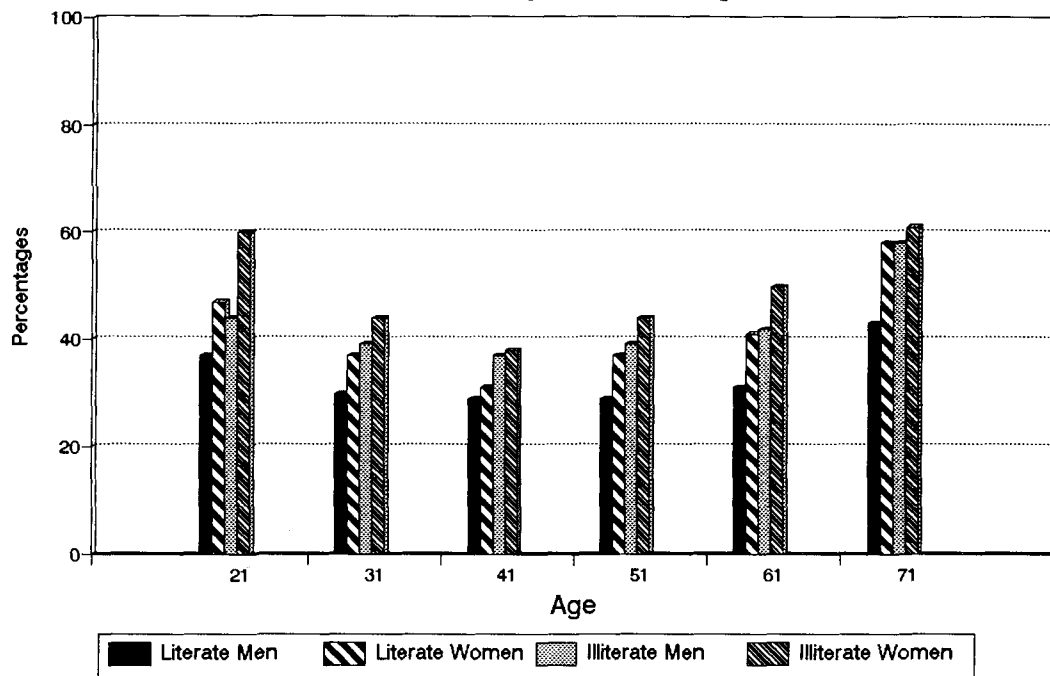
never voted. After the war, he was jailed, severely tortured, and sentenced to twelve years of prison, even though he had saved the life of one of the factory owners during the war.

Francoism, with its repression, was the darkest night of modern Spanish culture.¹⁸ Franco's victory in 1939 can be interpreted as the sacrifice on a grand scale of the militant, literate workers' elite. The great majority of other workers, mostly illiterates, were obliged by the vociferous and oft-repeated condemnation of the Republic and the war and the silence imposed on them to turn inward to survive, to "swallow" their own identity. But, paradoxically, the lack of schooling and literacy that barred them from effective integration into the system provided a defense against the vicissitudes of the revolution and repression, except in cases of those who participated in violent action or of women who lost their jobs or went into exile to follow their families. The reaction of the illiterate population to the repression was varied, subtle, and wise. The illiterates had the ability to lie silent, to pass unnoticed, to become invisible. In a certain way, their distance from power

¹⁸ Throughout Nationalist or Francoist Spain, all Masons, all members of Popular Front parties, all members of trade unions, and, in many areas, everyone who had voted for the Popular Front in the February election were arrested. Many were shot, and mourning was generally prohibited. Hugh Thomas, *The Spanish Civil War* (New York, 1961), 166.

Figure 4

Non Voting in the Popular Front by Literacy, Sex, and Age



A Sample of 24,748 Electors

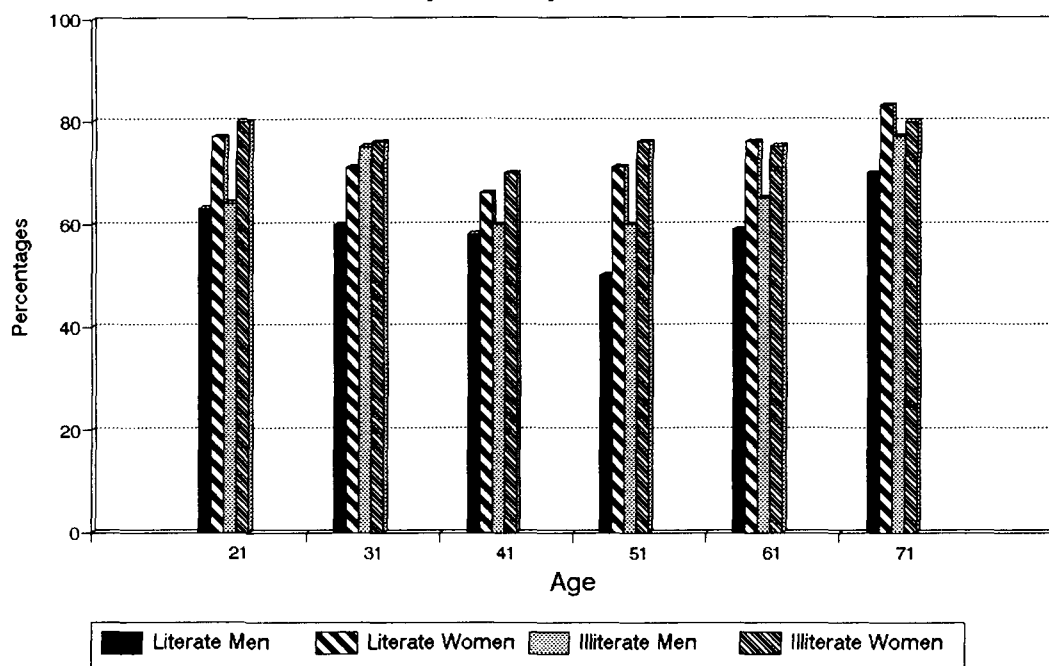
and the written culture saved them from the repression suffered by literate militants, and perhaps they maintained much the same attitude during the interviews we conducted with them.

The best thing to do was to be quiet, to go to your machine or your workbench and say, "I don't want to know anything about anyone," because if you confide in one person, and probably that one, like they used to come around at night, come forward, or whatever, they denounce you . . . and you had to keep your mouth shut.¹⁹

A great majority of the Spanish population was required to reject and forget its immediate past in order to continue living. Because of fear and censorship, the experience of the Republic and the Spanish Civil War was not passed on from one generation to the next. Democratic Spain after the 1976 transition rests on these foundations of repression, whose most notable traces remain the silence and fear imposed by the victors on the vanquished.

¹⁹ All the interviews are deposited in the Instituto Municipal de Historia, Sección de Historia Oral, Barcelona 08002.

Figure 5
Non Voting in the Compromisarios
by Literacy, Sex, and Age



A Sample of 24,748 Electors

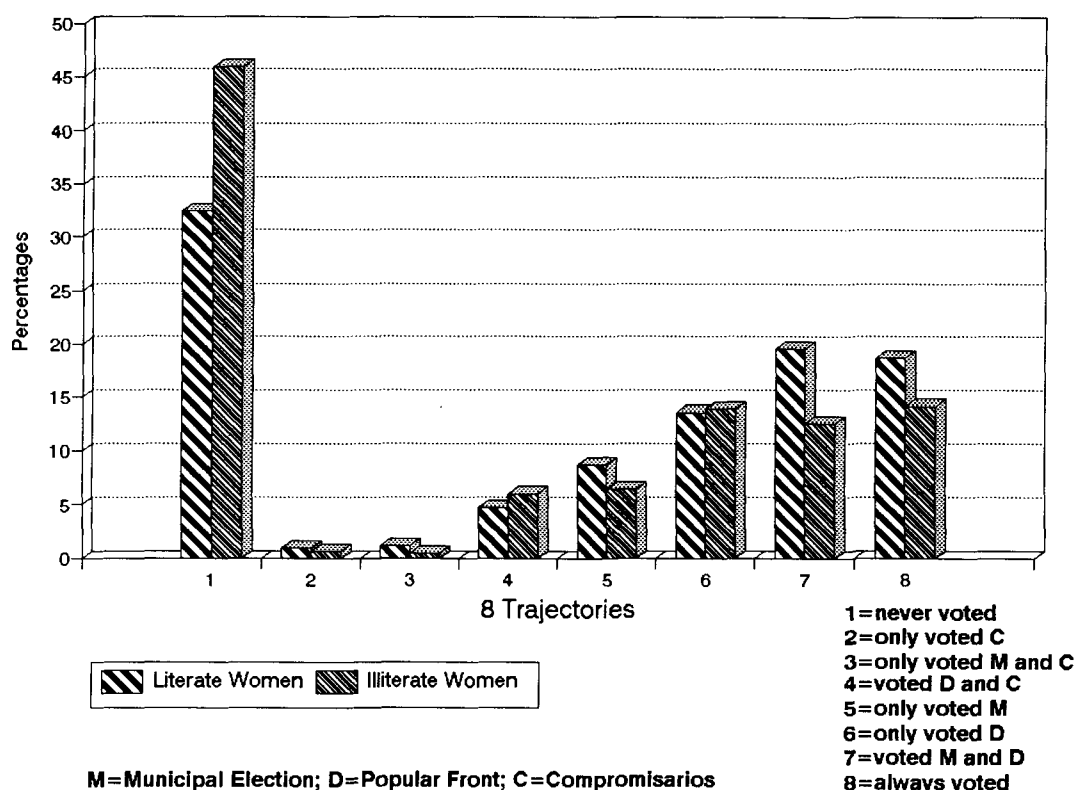
OUR INTERVIEWS WITH ILLITERATES²⁰ were confined to a very small group (nine women and three men), the only factory workers still surviving in the mid-1980s whom we could identify as illiterate. We compared them to a sample of twelve comparable literate persons. All were poor, blue-collar laborers, and in fact the literates rarely, if ever, wrote. It was the ability to read that distinguished the literates, who in general had only functional literacy.²¹ At the time of the interviews, in 1983 and 1984, they were elderly, between seventy-three and eighty-six years old.

In order to compare literates and illiterates, we took much care to conduct all interviews in the same way. We created a questionnaire and paid close attention to the formulation of questions and their diversity, alternating chronological questions (When did this take place?), with those about opinion (What did you

²⁰ Many of the ideas of the following paragraphs dealing with the interviews are taken from the draft of a French paper on which Dominique Willems and I are working. We have published some of our research in Spanish in the journal *Historia y fuente oral*, following the lines of an earlier version, "Lengua y poder en Cataluña" as cited in note 13 above.

²¹ Jeanne Chall distinguishes several stages in reading development. Functional literacy belongs to the third stage, defined as follows: "Functional literacy reading skills are sufficient to permit the reading of material that is fairly familiar, material that is within the reader's existing knowledge and meaning vocabulary. This includes the simple, non-technical parts of some newspapers and application forms, ads, signs and labels"; Jeanne S. Chall, *Stages of Reading Development* (New York, 1983), 120-21.

Figure 6
Trajectories for Women 1934-1936
by Literacy in Barcelona



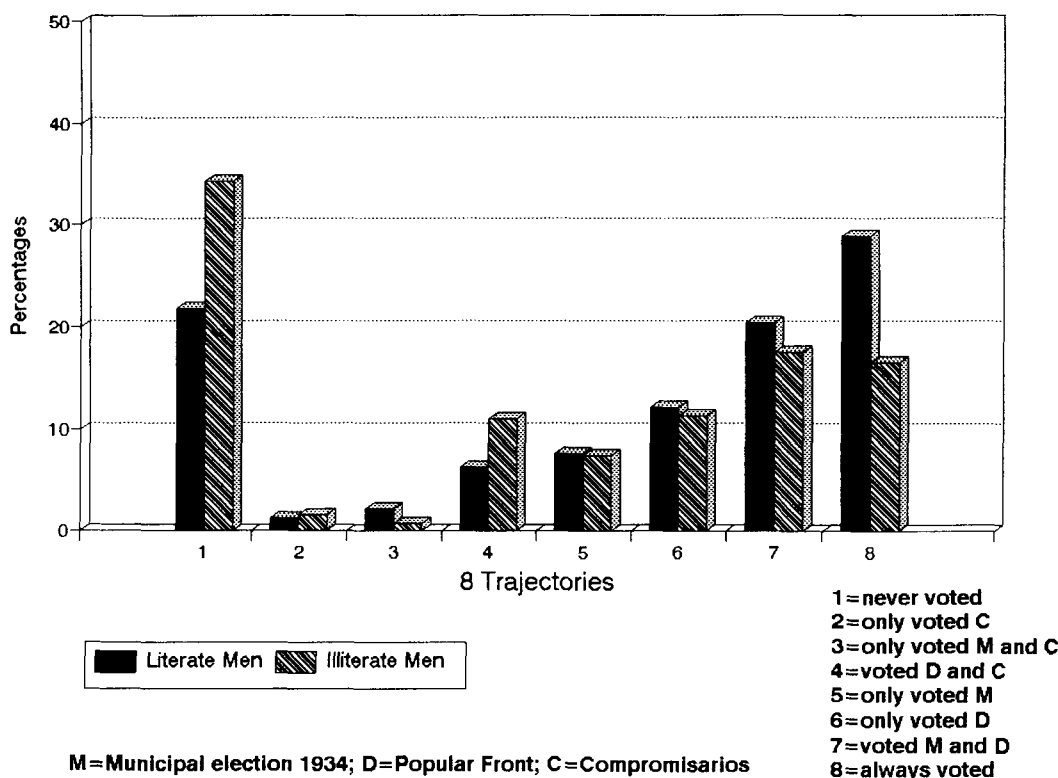
think, feel? What did it mean for you?), those about participation (What did you do? Why did you do it?), and those concerning channels of information (How did you decide? How did you know? With whom did you speak? About what?). We also realized that it was necessary to reconstruct the personal and communicational universe of each subject.²² Because of the difficulties in establishing a dialogue, we decided to conduct two types of interviews with each subject: first, we asked the subjects to relate to us their life story, and only afterward did we conduct a thematic interview with concrete questions about the elections and the civil war.

It was not easy to arrange the first interviews with illiterates. Excuses cascaded like waterfalls each time we tried to approach them by telephone. They would

²² The most difficult interviews (with illiterates strongly controlled by their children) lasted for twenty minutes. Successive interviews with illiterates who lived alone, in other words not controlled, sometimes totaled more than six hours. We always tried to conduct the interviews alone with the subject, but at times it was so difficult to get the person alone that we sent two interviewers. In this situation, we would begin the interview with all four parties together, but at the first possible moment, we would separate the two subjects and continue the interviews separately. In this way, we were able to collect complementary data on people who lived together.

Figure 7

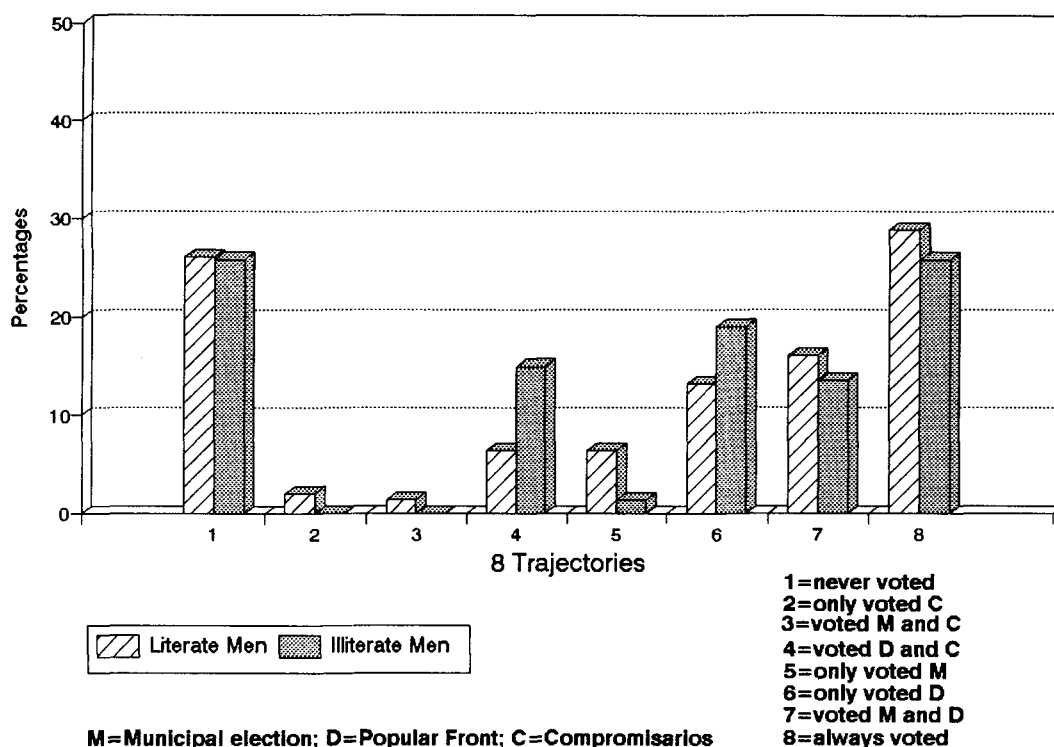
Trajectories for Men 1934-1936 by Literacy in Barcelona



become ill “all of a sudden,” were hospitalized, were paralyzed and could not open the door, or were about to leave on a trip. They all exhibited an amazing facility for improvising likely (and unlikely) excuses. Only when these were repeated over and over did we decide to go to their homes without forewarning them. But if we went directly to their homes, they would not usually answer the doorbell; if they did, they would not open the door; if they opened the door, they would not let us in. We had first to establish a series of relationships in order to find the person and gain access. For each subject, the path of approach was different. It sometimes took months to reach a person, as we slowly worked our way through pharmacies, parishes, stores, or neighbors to establish contact. The difficulty was associated with our theme—their lack of militancy—or with the control exercised over them by their families. Even today, it is hard for most Spaniards to talk on a personal level about the civil war and the Francoist repression. And it has been argued that those who feel they have not triumphed in life are those who least want to be interviewed.²³ The actual conditions of their lives, pensions, children, and homes

²³ Isabelle Bertaux-Wiame and Daniel Bertaux, *Jeunes villageois[s] de l'entre deux-guerres: Une enquête exploratoire*, in *Sociétés Paysannes et Depaysannisation, l'usage de l'histoire de vie en*

Figure 8
Trajectories for Men 1934-1936
by Literacy in Pueblo Nuevo



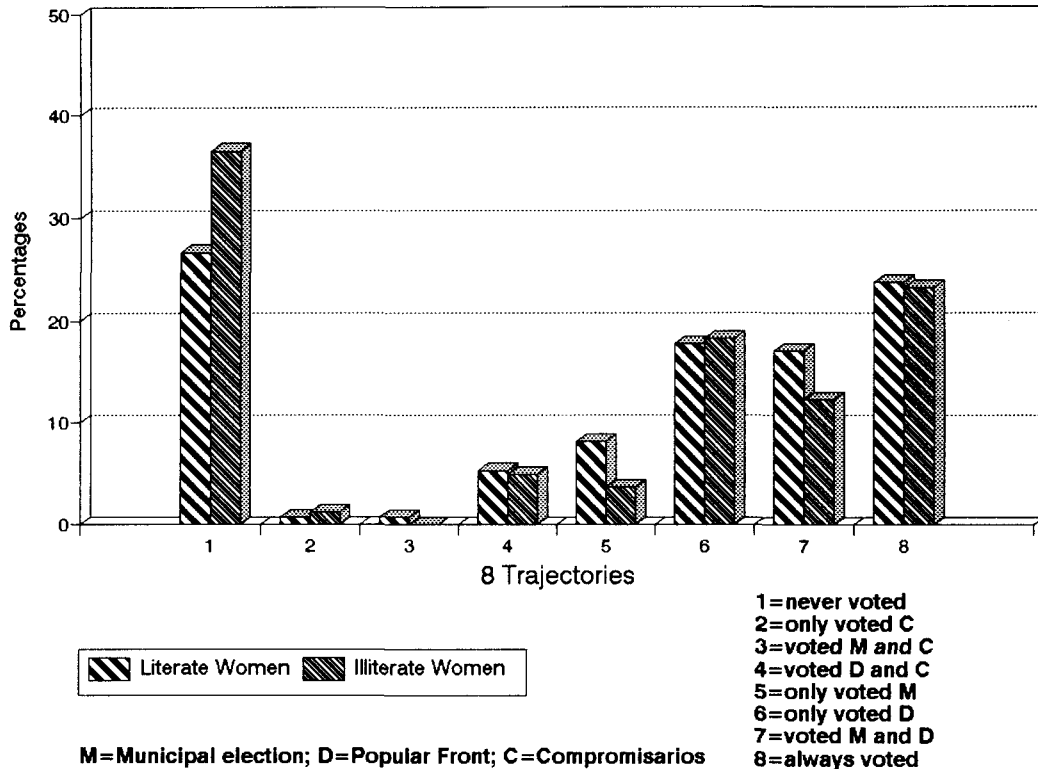
played a role in the degree of cooperation we received during the interviews and in the ease of access to them. Many people lived under the strict control of their spouses, siblings, or children. Even so, when we were finally able to reach them, many received us cordially and continued to do so as often as we wished to come, especially if they were single women who were not controlled by their children or husbands, women who had lived with a militant, or men who had achieved a moderately comfortable life.

After the first interviews, we noticed the difficulty that illiterates had in answering our questions, whether or not we had been welcomed by them. We immediately noticed a lack of cooperation, a distrust, even an aggressiveness as they explained, "This doesn't interest us" or "Leave me alone." Their aggressiveness increased as the interviewer insisted on a particular question, especially if it came from the questionnaire.

There was evidence of greater cooperation in the "life history" section of the interview in which the answers were longer and more complex. Although the

anthropologie et en sociologie, Tud Ha Bro, Sociétés Bretonnes 6, Université de Haute Bretagne, 1981: 94.

Figure 9
Trajectories for Women 1934-1936
by Literacy in Pueblo Nuevo



ideal dialogue may be based on the principle of cooperation between speakers who adapt to form a “collective speaker,” nothing like this occurred in the interviews with the illiterates. Rather, what we encountered was a conflict between two worlds, a dialogue of the deaf, as it were, in which the unequal relations of power were evident. This explains, in part, the aggressiveness and the lack of cooperation, and why the illiterates did not feel at all involved in the process. Nevertheless, the atmosphere was notably better in the sections on “life history”: the presence of the historian was less intrusive, and the illiterate, without leaving his or her communicational universe, was able to better share his or her story.

We realized that in the interviews we confronted a double handicap. First, historians are handicapped by concepts of little use in reaching illiterates. We use abstract and noncontextual jargon that makes communication difficult. Second, illiterates are handicapped in that they move uneasily in a reading culture not accessible to them. The illiterate memory is selective, as is all memory, and does not retain the “historical” facts, which have little pertinence and no real significance to them. Fundamentally, the illiterates’ distrust is a healthy reaction to a form of cultural and verbal aggression. But, probably even today, these attitudes reveal the fear they had during the repression of being branded by the police as

"red." By contrast, the literate workers were generally more willing to cooperate during their interviews; they seemed to feel more comfortable with the dialogue.

The illiterates speak just like the literates, and in a simple conversation it is not possible to uncover the differences between them. In the larger field of discourse, the testimony of the illiterates was generally characterized by brief responses, a total lack of any subjective modality in the discourse, and the control or absence of emotion (joy or pain). The majority had difficulty understanding and expressing abstract concepts, in particular political concepts such as the notion of a union, party, democracy, or socialism. Yet it is possible that in answering our questions they did not want to let us see that they understood, precisely in order to pass unnoticed. After half a century, our research on their behavior during the war could reawaken old fears, latent since the Francoist period of terror. Thus it is difficult to separate not being able to answer from not wanting to answer. (In the transcription of the interviews, Q = Question; A.Ill. = Answer by an illiterate person; A.L. = Answer by a literate.)

Q. "What is a union?"

A.Ill. "Well, everybody gets together, all the men, but nothing more."

Q. "And a party?"

A.Ill. "Less. I don't know."

Q. "Do you know what a political party is?"

A.Ill. "Well, look, I can't tell you anything, dear."

Q. "If I were to ask you which is better, a political party or a union, what would you say?"

A.Ill. "What do I know, dear?; I don't understand it."

Q. "Do you know what communism is?"

A.Ill. "Ah no, my dear, no."

Q. "Which do you think is better, fascism or socialism?"

A.Ill. "That, no, I don't understand that."

A.L. "Well, socialism is better."

Confronted with more "neutral" questions, they are not afraid to break their silence, they come out of hiding, and frankly admit that they do not understand:

Q. "And you, what does it mean to you to vote?"

A.Ill. "What do I know, I don't understand it, I don't understand it."

In our sample, there were also literates who did not understand, but they would ask for explanations. The illiterates never asked for explanations, and they did not try to understand, as if from the beginning they knew that they were not going to understand. Or, because they did not even want to broach the topic, they avoided asking. These types of questions aroused bad tempers, an air of distance,

or aggressiveness. The few definitions we were able to obtain were usually reduced to their most concrete aspects (to vote is to put a ticket in a box; to belong to a union is to hold a union card). Abstract terms, even if they were understood, systematically disappeared from their own speech; here, the difficulty in referring to these terms is evident:

Q. "Between Catholicism and Communism, which seems best?"

A.Ill. "Ah, well, the . . . the first that you said."

Q. "You have a good memory."

A.Ill. "I remember things well?"

The same happened with emotions and opinions, which they would not express directly. Either the illiterates did not answer at all or they evaded the questions. Taboos concerning personal relationships accentuated the dissymmetry between what they understood, what they felt, and what they could and would talk about. The barriers to communication and their silence were subjects that reappeared frequently: "I have always been quiet, I never talk"; "I can't explain, or talk like other women." Even illiterates who attended the assemblies spoke little, "No, no, I listened to them. They were more brilliant than I was at talking."

With respect to how the illiterates perceived events in their lives, the most common characteristic was their passive perception of the facts. Our subjects were content to verify events. Things happened to them, and they were passive observers, rarely the actors or participants. In spite of a marked egocentrism, they rarely appear as the subjects of their own stories; for them, the opposition between "us" (the passive) and "them" (the active) is a continuous thread that passes through all the conversations:

Q. "What did you think about strikes?"

A.Ill. "Nothing, what were we to think—they said 'strike' . . . 'go home' . . . we went home. And that's it. What were we to say? They were the ones that arranged things."

Q. "For how many persons did you take the rationing?"

A.Ill. "You stood in line, and then they gave it to you, if it arrived."

Q. "And did you go to the meetings?"

A.Ill. "But what for? I did not arrange anything. It was the same if I stayed or went. I did not go."

Rarely were we able to obtain the expression of an opinion, a feeling, a reflection. The illiterate lives in passive indifference: no compromise or fight but the acceptance of a fate that is usually adverse:

Q. "Do you think you are well paid, or should they pay you more?"

A.Ill. "Man, I don't know, they pay me what they pay me; it's all the same, I earn what they pay me."

Q. "And you did not feel exploited?"

A.Ill. "No, all the factories were the same."

Q. "What did the end of the war mean to you?"

A.Ill. "What do I know? The war was over, so the war was over. What were we to do? I don't know."

Q. "And when the war was over, what did you feel?"

A.L. "Well, calm; there weren't any more bombardments, or other things."

The pursuit of normality is another particularly striking characteristic of illiterates; they aspire to blend in with the rest of the people, to be like the "others"; they fear individuality and personal motivation. This search for "normality" is translated into the particularly frequent use of common themes, of topics and general truths, which may be used to hide their feelings and are sometimes contradicted in the course of the same interview.

Q. "For you, what did the end of the war signify?"

A.Ill. "Well, look, a great deal of joy among everyone. A joy for all . . . what do I know! If I don't have joy, I don't have anything."

This preoccupation with normality is equally evident with respect to electoral behavior: illiterates, even though they are poorly informed, say they vote like the rest of the workers. On this point, only the militant illiterate men have a somewhat different view: "You say to yourself you must go to vote, you say to yourself they [the candidates] will do a good job or not."

This desire to mix in with the rest, to hide, is coupled with a feeling of inferiority and an acceptance of it. The responses to questions concerning opinion, information, and commitment within the factory clearly exhibited this feeling of inferiority, which is usually explicitly tied to illiteracy (since I can't read, I can't answer) or gender:

Q. "Why did you vote?"

A.Ill. "Well, everyone went; well . . . well, I also had to go."

Q. "And the meetings?"

A.Ill. "No, no. I never went to the meetings, I never went. Don't you see that I don't understand, why should I go?"

Q. "The workers had meetings?"

A.Ill. "No, this they arranged among themselves; we did not, women did not count. Did not count for anything."

Q. "Did you see the news?"

A.Ill. "That's for guys."

Q. "Do you remember if you voted in '33?"

A.Ill. "Man, if the others in my house went to vote, I followed them; if they did not go to vote, I did not vote."

A characteristic that tends to differentiate illiterates and literates is the greater isolation in which the former live: they have few if any friends, even acquaintances at work, and there is a reduction in the space that surrounds them. Several of our subjects had never in their lives left their neighborhood, or been to a restaurant, or to the movies. Even today, they rarely watch television and then almost never the news. Their effective universe of communication is reduced to the immediate family. In the factory, there was a similar phenomenon of marginalization. Some of the literates said that they never, or rarely, associated with the illiterates, and the illiterates repeated that they were never informed about what was happening in the streets or in the factory.

Q. "You, in order to know what was happening [in the factory], who told you?"

A.Ill. "I, I, I worked and didn't worry about anyone."

A.L. "Well, among the workers."

Q. "And before the war, with whom did you talk?"

A.Ill. "Well, with no one."

Q. "And why were you silent?"

A.Ill. "Ah . . . well, I have always been silent. Let my grandson say: never, I never talk, no, no."

Some illiterates felt guilty about their inability to read: "It was my fault, I skipped school all the time." They were embarrassed. "When I began to work at Riviere, they said I had to sign in every week. And I said, I can't sign my name. It was very embarrassing, and then my sister, who knows more, taught me to sign my name." But for others this disability was less worrisome:

"I don't know how to read, I go through life half-cocked . . . I don't know, I'm not going to shoot myself. Well, I put up with it."

. . . That I can't read, well I haven't been informed about anything, that's all.

"We did not have to write to anyone."

"Ah . . . well, calm. I haven't suffocated, no."

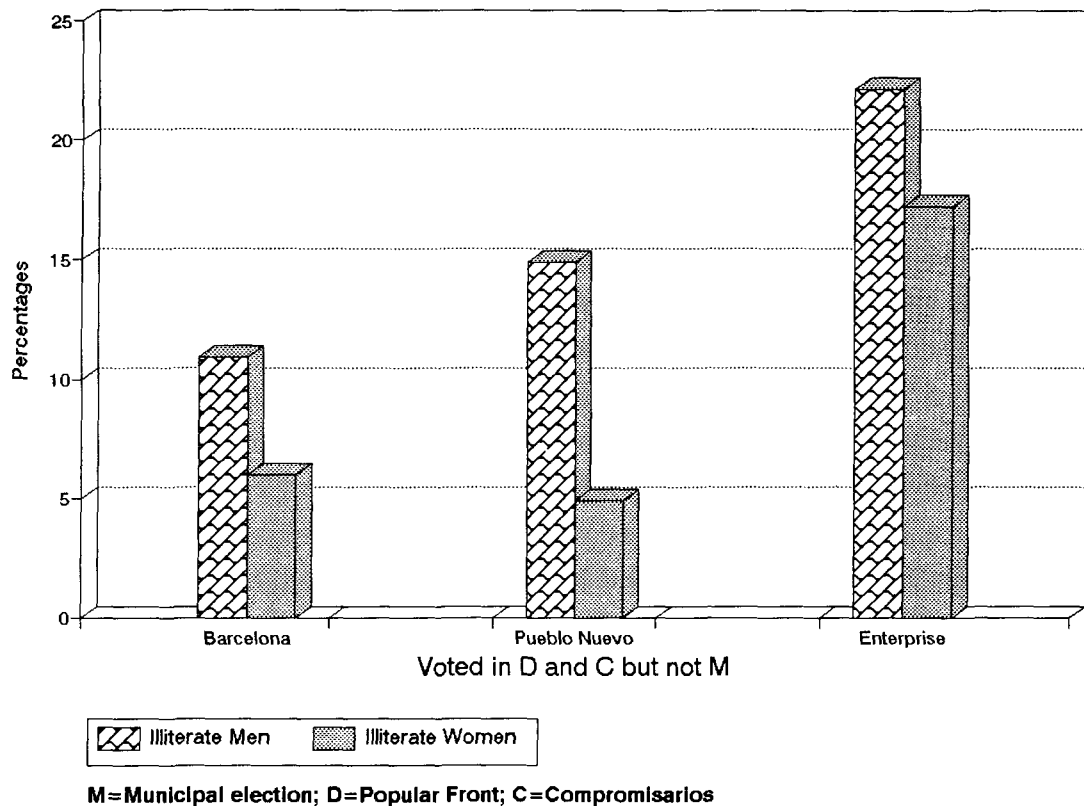
Illiteracy is often linked to being female or the situation of the parents:

"In the villages, they said that the men needed to know, but the women, even though they didn't know, it didn't matter, it wasn't important."

"Because they didn't know, they didn't teach us either."

FROM THE ELECTORAL POINT OF VIEW, the fact that illiterate men especially tended to vote in the least interesting election of the Republic indicates that there were two different worlds, particularly noticeable in the workplace (Figure 10). By

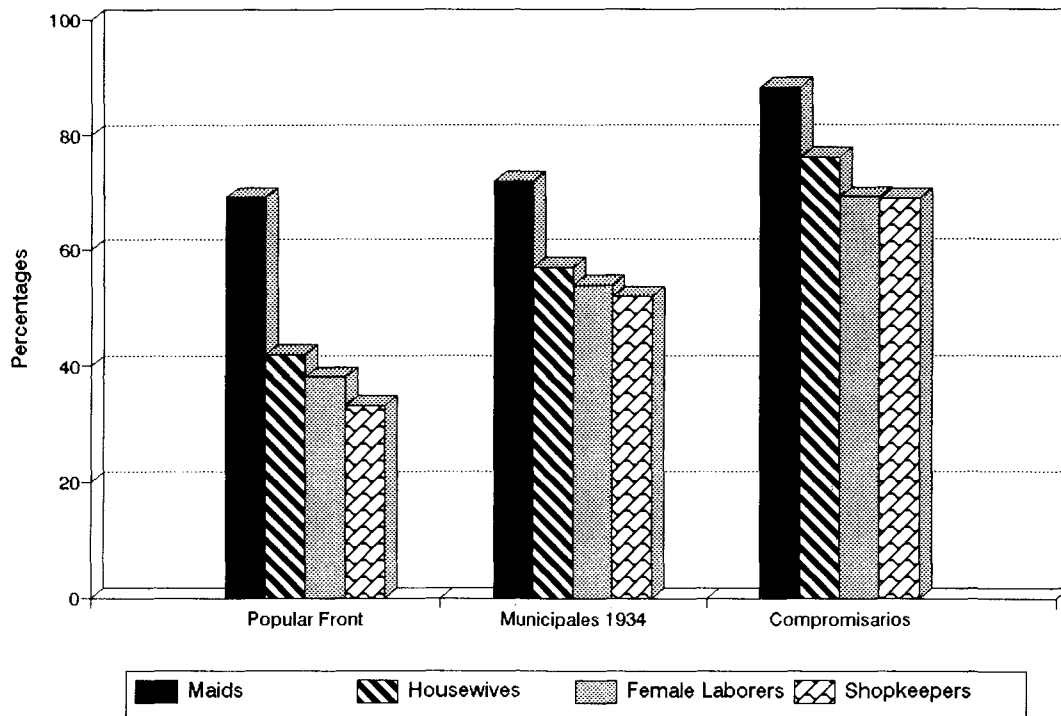
Figure 10
Singular Trajectory 1934-1936



contrast, we know that the illiterate male who wanted to be politically active was generally a young man who, unable to be a speaker (*orador*) or a writer (*escriba*) in the unions became a militant “shock-trooper.” During the war, such men were integrated into the militia and civil patrol groups, which conducted the most arduous tasks of vigilance and violence during the revolution. Another fraction of the men who followed this singular trajectory during the war probably later joined the socialist trade union (Unión General de Trabajadores, UGT) and were close to the Communist Party. Literate militants also belonged to the “shock-troopers,” but for the illiterates it was almost their only possible avenue to militancy. So, the few activist illiterate men there were did not fear becoming “visible,” although their violent actions caused them to suffer the harshest repression under Franco. Their illiteracy coupled with their youth and militancy gave their lives a tragic intensity; as illiterates who wanted to be politically integrated, they often paid for their integration with their lives. Their radical and fatal destiny is a paradigmatic example of the boundaries imposed on them by their non-literacy.

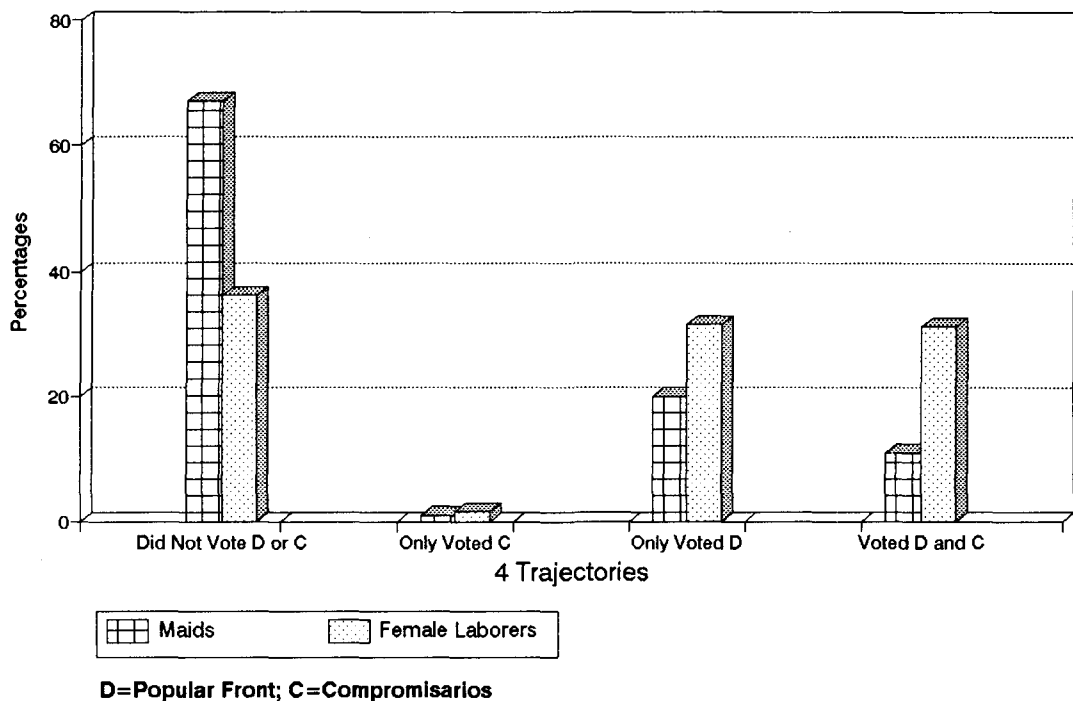
The few illiterate and literate women who tended toward militancy did so

Figure 11
Non Voting of Women by Occupation
in Barcelona during Three Elections



through the relations of dependency that they created with the leaders in the factories or through their familial relations. This dependency motivated them to follow either the extreme trajectories or the singular trajectory (Figures 9 and 10). Illiterate women from the factory abstained from voting less often or followed the singular trajectory to a much greater degree than the rest of the illiterate women of Barcelona. In other words, while the singular trajectory for women is not evident in the sections of Barcelona with high levels of illiteracy (such as Pueblo Nuevo, Figure 7), it is very evident in data from the factories. Women in the factories were electorally more integrated than illiterate women in the rest of the city—maids and housewives—who (see Figures 11 and 12) tended systematically to vote less often. The factory women paid with the loss of their jobs for having helped the workers' leaders and militants with the tasks of "collectivization." But, in general, women did not attend the assemblies; they remained silent and tried to ride out the storm. Even though we were able to interview only a limited number of illiterate men, we found important differences between the sexes. Not only is illiteracy more prevalent among women, but, upon classifying the interviews according to the degree of information that each respondent gave, we found that men, even those who had never been to school, apparently had more

Figure 12
Electoral Trajectories in 1936
Maids and Female Laborers



information about the political life of their workplace. Women were less informed.

Apart from gender, another decisive aspect of participation is whether illiteracy was solitary or shared among members of the same family, whether one or both parents were illiterate. Even more decisive is the degree to which the spouse was politically committed, because political commitment and the capacity for solidarity with others can break the barriers imposed by illiteracy.

All of the illiterates interviewed were poor, and none had gone to school. They had all begun to work as children in a world that did not require them to read or write. None said that illiteracy impeded them from working; yet, if they wanted to take an active part in union business, or even go to the silent movies, they had to learn to read. The lack of reading that circumscribed life at home and work, as much for men as for women, accentuated their marginality and their dependency. But other variables intervene simultaneously, such as poverty, lack of schooling, and isolation. Certainly, illiteracy is a characteristic that is linked with poverty—the difficulty arises in trying to distinguish the one from the other.

All the literate and illiterate blue-collar workers we interviewed in connection with the Riviere metallurgical factory study were also poor. A study of the electoral population of the Catalan village La Escala (population about 2,500)

during the Second Republic used income tax data that allowed us in turn to differentiate between poor illiterates and poor literates among the male population. We could not do the same with women, because the only females to appear in the income tax registers were widows. With respect to the male population, we could verify, as occurs in other countries, that poor men tended to vote less than rich men. But, in the case of La Escala, we also found that poor literate men voted more than poor illiterate men. As a result, we concluded that illiteracy added to the impact of poverty in producing electoral abstention.²⁴ In this sense, the statistics coincided with what the interviews told us.

It is important to keep in mind that the illiterates we interviewed were survivors and not necessarily typical. But it may be that their invisibility and ability to appear normal is a key to why few historical studies have been done on what it means to be illiterate. Illiterates are not really "seen." Moreover, there is the presumption that illiteracy is a residual condition, that it has disappeared or is rapidly diminishing, and that, in any case, it must be eliminated. Many researchers likewise believe that the determining factor of marginality is poverty, not illiteracy. Finally, perhaps most decisively, the invisibility of the illiterate population itself is also due to the fact that they do not use different speech codes and are therefore very hard to distinguish in casual conversation. Illiteracy is not an obvious characteristic, such as race, and does not tend to be the most obvious attribute of a group of people. Illiterates do not form groups; the most active, bright, or motivated illiterates cease being illiterate from their desire to attain literacy in adulthood through self-teaching.²⁵

THE "DISCOVERY" OF ILLITERACY and its differentiating characteristics should be a fundamental focus of oral history. For oral historians, illiterates constitute a new challenge. They are absent from all written documentation save statistics, and oral history seems to have the only method to integrate them into mainstream written history. The enormous difficulties in discovering and reaching illiterates, the communication problems, the refusals to answer, and the frequent silence often

²⁴ Mercedes Vilanova, "El abstencionismo electoral y su relación con las fuerzas políticas en la provincia de Gerona durante la Segunda República. Un ejemplo: La Escala." *Homenaje al profesor Joan Reglá* (Valencia, 1975), 2: 495–503. Tingsten, Dupeux, and Rosenstone reached a similar conclusion with respect to poverty. Tingsten in *Political Behavior*, 230: "The analysis of electoral participation of the different social classes has proved that as a rule, the political interest grows with rising social standard." Georges Dupeux, special edition of *Revue internationale des sciences sociales*, 12 (1960): 54: "Il y a donc bien une relation positive entre satisfaction et participation, et ce tableau donnerait quelque apparence de justification à la boutade: La politique est une luxe de riches." In the United States, Steven J. Rosenstone's conclusion in "Economic Adversity and Voter Turnout," *American Journal of Political Science*, 26 (February 1982): 42: "Economic adversity reduces voter turnout. The unemployed, the poor, and the financially troubled are less likely to vote." On the inferior participation of less educated people in the United States, see Raymond E. Wolfinger and Steven J. Rosenstone, *Who Votes?* (New Haven, Conn., 1980).

²⁵ In a study on the illiterate population in Brazil conducted in 1984, it was concluded that the illiterate population was incapable of exercising pressure because "the possibility to individually acquire status as a citizen via literacy made collective action among illiterates difficult, making them different from other groups who have historically applied pressure to secure their incorporation"; Maria Teresa A. Sadek and José Antônio Borges, *Educación y ciudadanía: La exclusión política de los analfabetos en el Brasil* (San José, Costa Rica, 1985).

make illiterates uncooperative and rebellious interviewees, especially in a society that has suffered a civil war and harsh political repression. These difficulties point to a new consideration of the possibilities and aspirations of oral history. For the illiterate, our scientific universe, our ways of thinking, our language, and our vocabulary constitute an obstacle, creating an almost unbreachable gap. The experience with interviewing illiterates permits us to measure to what degree the historian (even the oral historian) is anchored in the dominant, majority culture and is integrated into the power of writing. Illiterates, on the other hand, are marked by their non-integration and their position "away from power," in which any type of manipulation seems impossible. Our experience permits us to comprehend to what extent the "normal," "simple" interview is conventional, in the strictest sense of the word: a game that is determined by the limits of a culture, of a power, in which the expected response determines the form of the question.

Difficult or impossible dialogues require the interviewer to go beyond the limits of the questionnaire, to enter a different human landscape rich and unusual in its own right. From this point of view, the world of illiterates constitutes a relatively new field of research that requires new methods beyond statistics or written sources. The study of illiterates makes oral history truly oral, the orality of those who cannot read or write and who even speak little. But we as literates may be responsible for their silence. Our exclusivist way of thinking leads us to impose our way of life and our goals as superior, and we force them into invisibility. Thus it is not strange that we cannot see or hear illiterates. By ignoring illiterates, we lose a counterpoint necessary for understanding literacy, and we refuse to look at much of the past. We lose the complete memory of what we have been.

Review Article
The Multicultural Islands

PATRICIA NELSON LIMERICK

Greg Dening, **History's Anthropology: The Death of William Gooch**. Lanham, Md.: University Press of America, 1988. 122 pp.

Patricia Grimshaw, **Paths of Duty: American Missionary Wives in Nineteenth-Century Hawaii**. Honolulu: University of Hawaii Press, 1989. 246 pp.

Jocelyn Linnekin, **Sacred Queens and Women of Consequence: Rank, Gender, and Colonialism in the Hawaiian Islands**. Ann Arbor: University of Michigan Press, 1990. 276 pp.

David E. Stannard, **Before the Horror: The Population of Hawai'i on the Eve of Western Contact**. Honolulu: Social Science Research Institute, University of Hawaii, 1989. 149 pp.

"READ THESE FOUR BOOKS ABOUT HAWAII," a promotional brochure for the Armchair Travel Agency might say, "and explore the preeminent multicultural experience. Ponder the wonders and mysteries of human nature as you compare the curious habits of four exotic cultures."

The tour encompasses native Hawaiian culture before and after the arrival of Captain James Cook, English culture in the late eighteenth century, Protestant New England culture in the early nineteenth century, and international academic culture in the late twentieth century. While the habits and practices of all four cultures—the natives, the sailors, the missionaries, and the professors—are intriguing, not the least exotic of rituals are those practiced by the group closest to home: the academic historians and anthropologists of our own familiar era.

"Believing that all knowledge has a context," Jocelyn Linnekin begins *Sacred Queens and Women of Consequence*, "I endeavor here to clarify the context of this book and its author." The author, it turns out, is "an American woman with a personal commitment to feminism and civil liberties, a college student during the late 1960s, and now an anthropologist," and thereby a person likely to "see Hawaiian women as powerful actors and decision makers with great temporal and symbolic importance."¹

I would like to thank John S. Whitehead at the University of Alaska for his repeated efforts to call my attention to Hawaiian history. His essay, "Hawaii: The First and Last Far West?" will appear in the *Western Historical Quarterly* in May of 1992.

¹ Jocelyn Linnekin, *Sacred Queens and Women of Consequence: Rank, Gender, and Colonialism in the Hawaiian Islands* (Ann Arbor, Mich., 1990), vii–viii.

While the other authors provide their self-characterizations with a little more indirection, it would take a pretty dull reader to miss the clues. Greg Dening, in *History's Anthropology*, has self-revelation down to an art. Informed that Dening would pursue a Ph.D. in anthropology at Harvard, one of his history professors in Australia was dismayed: "'Dening,' he said, 'I have to say that I think this is the end of your academic career.'" Instead, Dening now has "the satisfaction of sitting as an anthropologist in a chair of history" at the University of Melbourne, drawing an unmistakable pleasure from the study of islands, beaches, borders, boundaries, and other "in-between" states of being. "If you want my ultimate liberal philosophy to be revealed," Dening writes, "let me say that I think it is the human condition to be in-between."² Also teaching at the University of Melbourne and the author of *Paths of Duty*, Patricia Grimshaw found that the conventional histories of the missionary movement in Hawaii had been written "as though men were the sole significant actors," with women, at best, appearing as "comic relief in a tale that might otherwise be a depressing one."³ Aware of the cultural inflexibility built into these women's minds, Grimshaw's empathy for their hardships and frustrations is also clear. While teaching history and American Studies at the University of Hawaii, David Stannard became discontented with the widely accepted presumption that only 200,000 to 300,000 natives inhabited the Hawaiian Islands before the Europeans came. *Before the Horror*, his critique of conventional Hawaiian demography, thus doubles as a searching attack on deference to authority in scholarly circles.

HAS HAWAIIAN HISTORY, THEN, FALLEN ON HARD TIMES? Have the scholars' personalities and politics crowded out the native chiefs and commoners, as well as the Euro-American sailors, traders, and missionaries? It is certainly possible to imagine readers who would make a quick trip to despair in response to this burst of authorial self-consciousness. There are, after all, still a significant number of historians who find the use of the first-person pronoun to be grounds for revoking a scholar's license. For determined believers in the writing of neutral, objective, bias-less, and passion-free history, these four books quietly announce the end of an era. "Who *was* that masked man?" the townspeople used to say after a visit from the Lone Ranger. In the late twentieth century, any writer of history trying to adopt the pose of the invisible, omniscient scholar is going to inspire the same question.

The glory days of the invisible, omniscient reviewer are also running short. Let me go on record, then, with two sadly contradictory biographical facts: first, for eleven years, I have failed to add material on Hawaii to either my American history or my Western American history survey courses, and second, I have been, for eight years, the Northern Hemisphere president of the Peripheral Studies Association. The first fact requires little explanation: it is no easy matter to add a lecture to a preexisting course; time is short, and it is hard to find the opportunity

² Greg Dening, *History's Anthropology: The Death of William Gooch* (Lanham, Md., 1988), 98–99.

³ Patricia Grimshaw, *Paths of Duty: American Missionary Wives in Nineteenth-Century Hawaii* (Honolulu, 1989), xix.

to read anything one does not “have” to read; the syllabus is already packed to the gills; the puzzle of Hawaii’s “non-contiguity” is always there to trip anyone in pursuit of a coherent course syllabus, with an ocean breaking up the sequence of one’s lectures. But the fact remains, when it comes to the sin of neglecting Hawaiian history, I rank with the best.

The second fact—my status as co-president of the Peripheral Studies Association—requires a little more background information. When I was an assistant professor teaching Western American history at Harvard, a number of my senior colleagues warned their graduate students away from my course because my subject was “peripheral.” Accordingly, when the Australian historian, Kenneth Inglis, came to Cambridge for a semester, his company was a great comfort, since the history of Australia made the history of the American West look central by comparison. In his company, the term “peripheral” began to develop some potential as an alternative source of identity and pride. Together, Inglis and I founded the Peripheral Studies Association, with Inglis as Southern Hemisphere president and me as Northern Hemisphere president. While the official membership has not grown much since the founding, the association does have stationery—letterhead with the PSA’s name and address printed in a narrow strip, set tightly against the far left edge.

For the founder of the Peripheral Studies Association to be so slow in paying attention to Hawaii is, indeed, surprising. As a blot on the record of a Western American historian, the neglect of the USA’s state in the mid-Pacific is a particular embarrassment. Many Western historians are now committed to the recognition that the United States is a bi-coastal nation, that North America is the transoceanic neighbor of both Europe and Asia, and that unbalanced attention to the European side of the neighborhood fails to tell the full story of our own continent. Western historians reject a version of American history centered on Massachusetts, New York, the District of Columbia, and Virginia. For us, there is only one good excuse for neglecting Hawaii, and that is fatigue, the fatigue of the scholar who cannot keep up with a thousand presumably more central subjects, and who thereby despairs of ever paying proper attention to distant islands.

This fatigue brings us to another vital matter of controversy in the world of academic history. These are, as everyone must have noticed, grumpy times in history departments. The grumpiness has registered most often in the public record as a debate over multiculturalism and Eurocentrism. Can great white male leaders share the spotlight with women, with workers, with villagers, with people from non-European points of origin? Can a “Eurocentric” way of thinking stretch to include the multiplicity of the world’s peoples and points of view? The answers to these questions do not come easily, and agreement does not come at all.

For reasons of personal and professional dignity, few of the participants in this well-publicized debate admit to the central pain of multiculturalism: the burden and embarrassment of not being able to keep up with one’s reading. Historians are consumers and producers in a market where production has wildly exceeded the capacity to absorb production. By one conventional lament, monomaniacal university administrators demand constant publication, and a flood of unreadable, scholarly trivia is the result. But no such luck. While dismissible trivia is well

represented in the pool, many books of merit appear as well. A quarter-century ago, living under a much narrower definition of historical significance, we were already too far behind to catch up. Now, with all the various places and peoples of the planet calling for our attention, we are lost. The information explosion will not relent; floods of books and articles continue to pour from the presses; and professors now have occasion to echo the familiar cry of their students: "I cannot keep up with my reading." Rather than admit to what might seem to be personal weakness, a number of academics prefer to express concern over the dangers multicultural history poses to national values.

IF ONE HOPED TO HAVE HAWAII provide one's first opportunity for appropriate, intentional failure to keep up, these books dash that hope. Two of them—Greg Dening's *History's Anthropology* and David Stannard's *Before the Horror*—raise such central questions about scholarly method, approach, and argument that they deserve the greatest professional attention. The merits of individual books aside, Hawaii is a premier case study in the intellectual dilemma—or opportunity—that we have come to call multicultural history. Before they could struggle over multiculturalism, historians had to become regular users of the concept of culture. Affliction or boon, that concept is a gift historians received from anthropologists, and scholarship about Hawaii is a prime example of the encounter of history with anthropology. Before the borders between them began to blur, anthropology tracked the natives, and history tracked the Europeans. In Hawaii, the natives and the Europeans met. Two centuries later, hot on the trail of their respective subjects, the anthropologists and the historians now cross paths and sometimes collide. In that encounter, particularly vivid when it takes place on an island, many of the puzzles of late twentieth-century historical writing present themselves for a reckoning.

In terms of those puzzles, Dening's *History's Anthropology* is one of a kind: a book about theory and method written with such charm and grace that even the prosaic-minded will cheerfully follow their tour guide through mazes of scholarly self-consciousness. The subject in the subtitle—"the death of William Gooch"—functions as the lure that keeps the pedestrian reader moving and motivated. The unfortunate William Gooch was a twenty-one-year-old Englishman, a recent graduate of Gonville and Caius College at Cambridge, sailing as astronomer on the ill-fated ship *Daedalus*. Commanded by Lieutenant Richard Hergest, the *Daedalus* was a supply ship sent in support of Captain George Vancouver's expedition to the Pacific. In May of 1792, having arrived late in the Hawaiian Islands and missed Vancouver, the *Daedalus* stopped at Waimea for fresh water. On a troubled voyage, Lieutenant Hergest and Astronomer Gooch had developed what we would call, in the late twentieth century, a difficult interpersonal relationship, and they took the opportunity presented by the landing party to stroll and talk out their differences. Cutting Gooch and Hergest off from the rest of the landing party, Hawaiians killed both men, as well as a Portuguese sailor named Manuel. The rest of the party made a quick retreat to the *Daedalus*.

As a peripheral man killed in a peripheral place, William Gooch was a perfect target for Dening's curiosity. Dening has "a file, indeed an old fashioned file, of those cards that one dare not bend, spindle or staple, on every sailor who came into the Central Pacific in British and American naval vessels between 1767 and 1842, some ten thousand of them." Of all those sailor-carrying vessels, the *Daedalus* was the most elusive; as a trading ship hired to naval service, it was "in-between, part-navy, part-trade," and was thereby less recorded or, in Dening's terms, less "texted" than ships more purely belonging to the navy. Since the *Daedalus* was "in-between," one can be sure that it would appeal to this particular author. Dening crossed oceans to track clues to the career of the *Daedalus*, examining records in Sydney, Australia; Wellington, New Zealand; Honolulu, Hawaii; Cambridge, Massachusetts; London, Cambridge, and Greenwich, England. The travels of the scholar, as much as the travels of the sailor, carry their ethnographic meaning: "Historians measure their own diligence," Dening notes, "by the miles of their trudging to see their primary sources."⁴

The trail leading to William Gooch began at the University Library at Cambridge, where his papers had been deposited. Always acknowledging the sketchiness of his evidence, Dening presents a portrait of a young man who began as the son of a wigmaker in the East Anglian town of Brockdish, who attended Cambridge with the help of benefactors, and who then, in an eighteenth-century version of networking, secured a position as astronomer on the *Daedalus*, leaving a mother, a father, and a fiancée (who "entered William Gooch's life only to say goodbye") behind in England.⁵ As one would expect of an anthropologist, Dening embeds Gooch's story in an examination of the peculiar and revealing customs of East Anglian villages, Cambridge colleges, and shipboard societies. If Europeans were Strangers to Hawaiian Natives, late eighteenth-century people are just as much Strangers to late twentieth-century people. Conscious, on every page, of the "Otherness" of the past, Dening offers not only a narrative of Gooch on his way to the beach at Waimea but also an analysis of British culture, Hawaiian culture, and the dynamics of their collision. In *History's Anthropology*, narrative and analysis do not retreat to opposite sides of the ring, to ignore each other or to prepare for a fight. The story of Gooch's short life, the story of native political affairs at Waimea and elsewhere in the islands, and the story of the late twentieth-century Greg Dening, obsessively on the track of Gooch, these compelling narratives serve to enrich and not to undermine historical analysis and reflection. Contrary to professional notions that analysis and narrative are intrinsically in opposition to each other, here they amiably inhabit the same book.

"So William Gooch was dead. What killed him?" Dening responds to his own question with an inspired list of suspects:

[Gooch's] going to Cambridge? His utilitarian spirit and his false sense of confidence in easily controlling his life? The intrusion of empire on native peoples? The greed for profit in the purchase of "artificial curiosities"? The ambivalent symbols of a ship part-navy, part-trade? The personal history of an erratic Hergest [the commander of the ship]? The peaks and troughs of a volatile friendship [between Hergest and Gooch]? The ambiguities

⁴ Dening, *History's Anthropology*, 24–25, 25, 28.

⁵ Dening, *History's Anthropology*, 72.

of Waimea, a beach between Native and Stranger, a beach in Hawaiian polity? The politics of chiefs? The accidents of misconstrued signs?⁶

Thanks to Dening's success in establishing the down-to-earth implications of abstractions that, in the hands of many other writers, seem inconsequential, all of these factors strike the reader as distinct possibilities. *History's Anthropology* thus concludes as a one-of-a-kind murder mystery: the mystery is never fully solved, but the reader is surprisingly content and not at all inclined to sue the author for failure to conclude.

DAVID STANNARD'S *Before the Horror* also falls within an expanded category of murder mystery. Like the Indian people of the Western Hemisphere, Hawaiian natives experienced a high death rate and a low rate of reproduction after contact with Europeans. The native Hawaiian population plummeted in response to the introduction of new diseases. Could this, by any stretch of the imagination, be murder? In the opening epigraph of the book, Stannard quotes William Anderson, Captain Cook's surgeon, describing the introduction of syphilis to Tonga: "The man who has rob'd, murder'd and been guilty of all the Catalogue of human crimes is innocent when compar'd to the one who [introduces a devastating disease] knowingly," words written, Stannard notes, "18 months before visiting Hawai'i and knowingly bringing [syphilis] to the Hawaiian people."⁷

However one distinguishes inadvertent injury from conscious murder, the fact of the native population decline is not in dispute. The dispute instead involves the scale of the decline. How many people lived on the islands before Europeans arrived? Conventional estimates calculate 200,000 to 300,000; but David Stannard, following the example set by Woodrow Borah, Sherburne Cook, and Henry Dobyns in revising the estimates of North and South America's native population, reckons 800,000.

This is not a subtle difference. Acknowledging the "speculative and controversial nature of [his] findings," Stannard knows he has to make a careful, methodical argument in order to support a conclusion so ambitious.⁸ The result is demographic prose remarkable for its clarity. Regardless of how they appraise his reasoning, quantitative historians could profitably follow Stannard's example in taking every step to ensure that difficult technical prose will not rob him of his audience.

In Hawaiian demography, all estimates—high or low—rest on an interpretation of the first written observations of the island population, remarks made by James King, who sailed with Captain Cook. King completed Cook's three-volume narrative of his journeys, published in 1793. An alert observer, King looked at the coast of Kealahou on the island of Hawaii, and, on the basis of that observation, ventured "a rough calculation of the number of persons" in the islands as a whole,

⁶ Dening, *History's Anthropology*, 94.

⁷ David Stannard, *Before the Horror: The Population of Hawai'i on the Eve of Western Contact* (Honolulu, 1989), vii.

⁸ Stannard, *Before the Horror*, xiv.

arriving at a total of 400,000.⁹ Taking this passage as their raw material, demographers have mined and refined it to arrive at their own estimates. Stannard organizes his appraisal of King with a step-by-step analysis of King's four assumptions: first, that "the population of Kealakekua Bay was about 2400 people, making for a population density of 800 persons per coastal mile"; second, that "the population density of Kealakekua Bay was approximately the same as that prevailing along the entire inhabited coastline of all the Hawaiian islands"; third, that "about a quarter of all the islands' coastlines were uninhabited"; and fourth, that "only the coastlines were inhabited," with "no inland population."¹⁰

Like a mechanic taking apart a faltering engine, Stannard dissects each of these assumptions. For each, he makes a convincing case that the proposition is, at the least, doubtful and that a proper appraisal of its implications would lead to an upward revision of the general population numbers. Taking one last shot before resting his case, in a chapter called "Some Likely Objections," Stannard anticipates and answers three possible criticisms of his conclusions. The growth rate required to transform a small population of early Hawaiian settlers into a population close to a million is, Stannard argues, well within the realm of probability. The Hawaiian physical environment offered natural resources sufficient to support a population of this size, thus scarcity would not have forced the population into a decline before the arrival of Cook. And, from the evidence of examples drawn from a variety of other native populations in Polynesia and the Western Hemisphere, the steep rate of population decline required to produce historically recorded low levels was well within the demonstrated range of possibility. The burden of proof, Stannard concludes, is now on the defenders of the conventional, low estimates: "it is now incumbent on those who would hold this position to demonstrate—in specific scholarly detail—precisely how [the population] came to be less than what all the evidence suggests is a minimum."¹¹

Publishing Stannard's argument, the Social Science Research Institute of the University of Hawaii invited appraisals from Eleanor Nordyke and Robert Schmitt, "generally considered the leading demographers in the study of Hawaii's population and proponents of the belief that the pre-1778 population was between 200,000 and 300,000."¹² Curiosity—about the flaws that these experts will find in Stannard's case, flaws that the lay reader is probably unprepared to detect—makes one turn to the second part of the book with eagerness. The intellectual satisfactions of following Stannard's argument are vivid and memorable; if the opposing team plays anywhere near as well, the reader thinks, this will be an intellectual contest to remember.

Nordyke and Schmitt, however, play another sport entirely. Instead of matching Stannard's crystal-clear, point-by-point, level-by-level manner of building his case, Nordyke's and Schmitt's responses hop in seemingly random order from topic to topic. They pay Stannard's essay a perfunctory compliment or two. It is "extensively researched," "well-written," "an interesting intellectual exercise," an

⁹ James Cook, *The Three Voyages of Captain James Cook Round the World*, vol. 7 by James King (1793; rpt. edn., London, 1821), 118–19, quoted in Stannard, *Before the Horror*, 3–4.

¹⁰ King, in Stannard, *Before the Horror*, 14.

¹¹ Stannard, *Before the Horror*, 80.

¹² In Stannard, *Before the Horror*, 103.

"impressive monograph," and "a far-ranging, comprehensive and heavily documented review of all the evidence."¹³ They then repeat old assertions, assertions seemingly undermined by Stannard's critique; but neither Nordyke nor Schmitt explains why those assertions still hold their loyalty. Stannard, for instance, had made a convincing case in support of the proposition that evidence for the widespread practice of infanticide was extremely limited and that, in any case, infanticide could not have been practiced on a scale large enough to have a major impact on the population. Rather than responding to Stannard's critique, Nordyke simply quotes, without any appraisal or commentary, from the native historian, Samuel Kamakau: "Infanticide was another evil practiced in pagan days."¹⁴ The source raises inescapable questions of credibility. As Stannard describes it, the passage comes from "a missionary-edited and translated newspaper article written in 1867 by a Christianized and evangelical Hawaiian chronicler."¹⁵ Yet it seems to be Nordyke's assumption that just quoting Kamakau fully answers Stannard's three-page argument for discounting the impact of infanticide.

At the end of their brief essays, both Nordyke and Schmitt announce themselves unconverted. Although a list of quotations from authorities holding to lower estimates "hardly constitutes a convincing refutation of Stannard's main points," Schmitt concedes, "it does demonstrate a widespread conviction among past analysts—many of whom were obviously well qualified to offer an opinion—that the 1778 total was lower, and not higher, than the estimate of 400,000 published by King." The low estimates "have been accepted by a number of modern authorities, all of them far more eminent than the present author, and several still living," writes Schmitt. Schmitt, writes Nordyke, is a fairly eminent soul himself: he "has pursued meticulous research on Hawaiian subjects for over 40 years," "worked in conjunction with other Hawai'i population authorities," and "published an array of books and articles with thorough documentation." Schmitt's "breadth of knowledge and his alliance with other population scholars," Nordyke feels certain, "offer credence" to his figures. "I must reject the suggested figures given in this paper," Nordyke concludes, "and endorse the previous findings of Hawaii's authorities of history and demography."¹⁶

The reader who is, by no one's definition, an "authority" in this field ends up puzzled. Why did Nordyke and Schmitt retreat into calls for deference to scholarly authority, expertise, and eminence, instead of taking on the substance of Stannard's case? Did a great leap upward of 500,000 strike them as so inherently impossible that a closely reasoned response simply was not necessary? Or did they fail to answer Stannard's argument because Stannard's argument is unanswerable? Is there a powerful case to be made against the estimate of 800,000 Hawaiians, and did Nordyke and Schmitt, for some unspoken reason of scholarly gentility, decline to make it? What is the nonspecialist to think, when the opposition forfeits the match?

¹³ Eleanor C. Nordyke, "Comment"; and Robert C. Schmitt, "Comment," in Stannard, "Part Two: Critical Commentary and Reply," *Before the Horror*, 105, 114.

¹⁴ Nordyke, in Stannard, *Before the Horror*, 111.

¹⁵ Stannard, *Before the Horror*, 138.

¹⁶ Schmitt, in Stannard, *Before the Horror*, 116, 115; Nordyke, *ibid.*, 106, 113.

For the reader puzzled by this curiously incomplete dispute, watching Schmitt and Nordyke bow to scholarly authority and reputation as if to royalty, Greg Denning's ethnographic angle of approach to scholarly behavior comes to the rescue. "Examination, grading, peer review, employment, promotion, tenure, publishing requirements," Denning notes, "are effective sanctions in the educating process to bend the historian's will to what proper history might be."¹⁷ But that careful, thorough, ritual conditioning meets its match when the accepted world-view gets a shaking as thorough as the shaking Stannard gives conventional Hawaiian demography. No wonder Schmitt and Nordyke write in the manner of property owners in a neighborhood where an unmannerly intruder has suddenly taken up residence. Beyond reminding the intruder to respect authority, it is not clear what the established residents can do to restore the lost quiet and calm of the old neighborhood.

More important, Schmitt's and Nordyke's disarray is a perfectly predictable symptom of life at the borders of anthropology and history, at the intersection of the past of an oral culture and the past of a literate culture. It is simply very difficult to get and to keep one's bearings when "hard" evidence is so elusive. There was no census taken in the Hawaiian Islands in 1778. The problems involved in a "retrocast," estimating a population "long before the first firm data point," as Robert Schmitt puts it, are "as formidable" as the problems involved in a forecast; "sizeable errors" prove "almost inevitable."¹⁸ Schmitt wrote in a 1971 study that "the evidence . . . is admittedly sketchy and judgment plays an uncomfortably large role."¹⁹ With or without the help of computer modeling and the finest quantitative reasoning, scholars will not solve the mysteries of the Hawaiian side of the story. William Gooch, Denning notes, "could not read . . . the History [the Hawaiians] made of the events in which they were conjoined with him." The dilemma of Strangers baffled by Natives has not gone away; "if I could have done" for the Hawaiians who killed Gooch "what I have done for Gooch," Denning laments, "I could have written well-rounded History. But I cannot offer my half guesses about them as something surer."²⁰ Stannard, Nordyke, and Schmitt strain to discover a past without the written texts that give historians their moorings. Where oral cultures and literate cultures meet, guesswork is the name of the game. At this stage of the match, Stannard has pulled ahead by offering what could function as a manual on "How to Apply as Much Reason as Possible While Guessing," while Nordyke and Schmitt offer something closer to a guidebook on "How Younger Scholars Should Respect Their Elders."

LIKE DAVID STANNARD, Jocelyn Linnekin disagrees with her predecessors in Hawaiian scholarship, and, like Stannard's *Before the Horror*, Linnekin's *Sacred Queens and Women of Consequence* is a case study in the ways in which a different

¹⁷ Denning, *History's Anthropology*, 23.

¹⁸ Schmitt, in Stannard, *Before the Horror*, 119.

¹⁹ Robert C. Schmitt, "New Estimates of the Pre-Censual Population of Hawai'i," *Journal of the Polynesian Society*, 80 (1971): 237-43, quoted in Stannard, *Before the Horror*, 5.

²⁰ Denning, *History's Anthropology*, 88, 89.

kind of reading can reverse conventional interpretations of familiar evidence. As Linnekin remarks, "symbolic material always permits alternative interpretations."²¹ But, unlike Stannard and Dening, Linnekin raises another puzzle of late twentieth-century academic behavior: why do scholars, who clearly care a great deal about their subjects, make it so hard for readers to read?

Sitting down with these four books, I picked up *Sacred Queens* first. In the manner of Goldilocks choosing porridge and sleeping accommodations, and judging purely by titles, I thought *Before the Horror* seemed too disheartening, *Paths of Duty* seemed too predictable, and *History's Anthropology* seemed too dull. So I would start with this apparently fascinating reconsideration of the position and status of Hawaiian native women. Did the *kapu* system, segregating males from females at meals and denying women—at penalty of death—the right to eat high-status foods, indicate a general subordination and devaluation of women in native culture? The records of land distribution in the 1840s reflected an increase in land ownership by women; did that mean Hawaiian women had risen in status just as native society was, in general, undergoing a decline? Did the female experience of conquest and the male experience of conquest thus diverge in crucial ways? These seemed fascinating questions. But an hour with *Sacred Queens* rearranged my priorities. I shifted to *History's Anthropology* and learned that an enormously lively book can carry a dull title. I went next to *Before the Horror* and discovered that demographers can write accessible prose. I moved on to *Paths of Duty* and, thanks to Patricia Grimshaw's poignant and telling quotations from missionary women, remembered why letters and diaries are treasured sources for historians. Then, facing up to my own path of duty, I returned to *Sacred Queens* and reacquainted myself with the bewildering, cryptic, and discouraging prose of social scientists.

The ungainly sentences of this book are a constant source of surprise and perplexity. The author did an enormous amount of work to produce this book, tracking down land records, assembling and analyzing data, coordinating her primary source findings with the standing secondary source literature. Yet, and here is the mystery, Linnekin wrote up the results of this considerable labor in a manner guaranteed to discourage all but the most determined and committed of readers. Linnekin relies on the standard social-scientese mechanisms for fending off readers—polysyllables, passive voice, and jargon. She has a few curious habits of her own, a fondness, for instance, for the word "sediment," offered as a verb and not as a noun, and used repeatedly where the rest of us might say "to cement." Here, as in many other scholarly books and articles, an unspoken assumption rules: any charm or grace in prose will erode the seriousness of the study, while dense, difficult phrasing certifies the content as substantial and sophisticated.

The reader who persists will find an intriguing case study in the feminist reappraisal of familiar historical assumptions. The *kapu* system, separating men and women at meals and denying women certain foods, seemed to carry a clear implication in the subordination and devaluation of women. Indeed, Linnekin

²¹ Linnekin, *Sacred Queens and Women of Consequence*, 19.

argues, elements of native religion did favor men's interests over women's. But some myths and legends testified to a deep respect for women's power. In any case, there is a good chance that women and men responded to formal religion in different ways. Even if official, priest-administered religion tried to tell women they were inferior, there is no guaranteed evidence that they fully accepted that message.

It would be, moreover, a serious error to assume that religious strictures told the whole story of women's status in Hawaiian society. Both the world of ideas and the world of behavior and of material reality must enter into the calculations. Looking outside the realm of formal religion, Linnekin finds a range of evidence indicating that native society did not consistently devalue women. In a stratified society, the nobility was composed of female as well as male chiefs, and the female leaders received much the same deference accorded to the male leaders. By its separation of the sexes, the *kapu* system gave women their own domain of influence. If the *kapu* system kept women from eating certain valued foods, it also denied male commoners many of the same privileges. In their encounters with European and American sailors, native women exercised a considerable range of options in offering sexual favors to the men on the ships and sometimes openly defied the interests of native men. Women controlled the production of cloth and mats and benefited by their association with these high-status goods. Finally, in her most interesting data, Linnekin traces a pattern of increased inheritance of land by women. In the process of Euro-American conquest, with an accelerating pattern of individual mobility, in and out of family residence groups, the decision to give land title to women rested on their role as caretakers and custodians of family coherence. Conquest devalued Hawaiian people in general, and yet conquest also might have meant a rise in the power and status of women or at least a deepened recognition of their vital role as sisters in a kinship network held together by sibling relations.

Before reaching these conclusions, Linnekin made a full disclosure of her status as a late twentieth-century feminist. In the same vein, she notes that her influential predecessor in Hawaiian studies, anthropologist Valerio Valeri, had his own context: Valeri's "characterization of Hawaiian women as 'marginal' and 'passive' in the context of the sacrificial religion strikes me as a characteristically Western male view of women and moreover, if I may be forgiven for indulging in a stereotype myself, a very Mediterranean male attitude."²² Differences in the background of scholars aside, there are, unquestionably, hard data at stake here. The Great Mahele—the project in the 1840s of confirming individual native land titles in Hawaii and opening the way to non-native ownership—provided the occasion for a flow of records on landholding, and Linnekin has put the data from those records through the mill of computer analysis. But, as she remarks, the computer provides no escape from human subjectivity: "it should be noted that I formulated the initial problematic, designed the computer database, constructed the analytic categories, supervised the coding of the land data, and wrote the programs to elicit inheritance patterns over time. I also designed the tabular

²² Linnekin, *Sacred Queens and Women of Consequence*, viii.

presentation of the statistical results for this book. Quantification, in other words, is no more context-free than qualitative analysis."²³ Read by one kind of reader, the evidence of Hawaiian history yields a portrait of native women subordinated and degraded by their own society. Read by another kind of reader, the evidence yields a portrait of native women with a considerable domain of self-determination.

FOR THE EIGHTY MISSIONARY WOMEN WHO TRAVELED FROM New England to Hawaii in the first half of the nineteenth century and who are portrayed in Patricia Grimshaw's *Paths of Duty*, uncertainty on this issue was not among their many burdens. Heathen society, the missionaries knew, left women "desperately degraded," "drudges . . . for their lords and masters, slaves . . . to male sensuality."²⁴ One of the principal aims of mission work was to remove Hawaiian women from degradation and raise them to the level of respect and value enjoyed by Anglo-American women. Hence the irony explored by Grimshaw: rather than liberating Hawaiian women from the confines of "heathen" life, American women found themselves sidelined in the mission enterprise, constrained in part by New England cultural conventions that handed leadership over to males and even more by the physical strain and unending obligations of childbearing and child rearing.

The path of duty was supposed to run straight, but instead it twisted, turned, and led its travelers to destinations they neither anticipated nor wanted. The path began at the most curious of starting points, the rushed marriage of virtual strangers. It seems doubtful that any "heathen" society practiced a more peculiar method of choosing a spouse than the feverish search of a male missionary who could not serve until he came up with a wife. Unmarried male missionaries, off on their own in Polynesia, would be men facing various temptations all too easily imagined. Moreover, the introduction of proper family life was a key element in the conversion of pagans, and the best way to teach family life was to demonstrate it. Male candidates for the missionary field thus learned that the final test in their candidacy would be the securing of a wife. With the pressures of scheduled departures, this choice could not be a matter of leisure and reflection.

The stories of these courtships thus press at the limits of probability. Proper New England women, barely introduced to their suitors, had to decide if they would leave their homes and families to spend their lives with these (to use Greg Denning's term) Strangers. "I think I can get one," Bethuel Munn declared at the start of his search for a wife in 1836, and the quest soon yielded Louisa Clark, acceptable despite the fact she wore glasses and was "easily fatigued in hot weather."²⁵ In 1831, Fidelia Church and Titus Coan corresponded over the question of their possible marriage but did not strike immediate harmony. Fidelia expressed her support of the right of women to pray aloud in religious meetings; Titus disapproved and "sent her two tracts opposing women's public praying."

²³ Linnekin, *Sacred Queens and Women of Consequence*, ix.

²⁴ Grimshaw, *Paths of Duty*, xiv.

²⁵ Grimshaw, *Paths of Duty*, 11.

Despite this inauspicious exchange, Fidelia and Titus married and set off to bring light to, in Fidelia's words, the "moral night which rest[s] upon the hearts of the untaught, wretched heathen."²⁶

The path of duty was instantly a rough one: a trying five-month sea voyage, encounters with the baffling and sometimes distressing ways of Hawaiian people, the necessity (hard on democratic New England sorts) of courting the favor of chiefs in order to secure access to the commoners, adjustments to an unfamiliar diet, climate, and landscape, housekeeping under difficult circumstances, separation both from family and friends at home and from the other missionaries on the islands, and, for most of the women, a relentless pace of childbearing. "Their presence in foreign mission service," Grimshaw notes, "was part of a separate female ambition for an important and independent career."²⁷ In practice, the missionary women felt more and more marginalized, relegated to a private sphere of housekeeping and child rearing. Facing up to her limited opportunities to work directly with the Hawaiians, Sybil Bingham summed up how the original "separate female ambition" diminished to wifely support of the husband: "I have this consolation—I am allowed to aid one whose constant employment is in the way of direct efforts for [the Hawaiians'] good."²⁸

According to the women's initial vision, this segregation of wives from the central mission effort would not have arisen. Hawaiian children and Anglo-American children would have studied and played together, supervised and guided by the mission women. But the problem was not simply one of too little time to divide between family and teaching. Left to their own choices, Anglo-American children preferred the customs of their Hawaiian neighbors; instead of the American children uplifting the Hawaiian children, the Hawaiians seemed, to the missionary parents, to be degrading the Americans. At the core of missionary fears was the problem of sexuality. To anxious missionary parents, the company of Hawaiian children constituted a direct and unambiguous invitation to sin and depravity. And so the missionaries quarantined their children. The dreary hours passed by Mercy Whitney's two-year-old daughter Maria summed up the pattern: "Much of the day she sat at the bedroom window watching the Hawaiian children at play nearby, without attempting to join them."²⁹ The segregation of the children also meant the segregation of their mothers. If Grimshaw's study can sometimes make the reader feel like throwing open the window or going for a walk, it is because the sources that she draws on so effectively convey the sense of cloistered, fatigued women, unhappy in their so-called homes. "We are hemmed in on all hands," wrote Fidelia Coan.³⁰ While Coan was describing the difficulties of travel in a landscape bounded by mountains and ocean, the same phrase could convey the position of women confined by the obligations of domestic work, by their discomfort with the company of their native neighbors, and by "the unequal balance of power" in their marriages.³¹ When Fidelia Coan, who had once

²⁶ Grimshaw, *Paths of Duty*, 21, 1.

²⁷ Grimshaw, *Paths of Duty*, xxi.

²⁸ Grimshaw, *Paths of Duty*, 44.

²⁹ Grimshaw, *Paths of Duty*, 47.

³⁰ Grimshaw, *Paths of Duty*, 52.

³¹ Grimshaw, *Paths of Duty*, 74.

engaged her suitor in spirited debate over the right of women to pray in public, bemoaned reports of “females acting in public” on behalf of abolition (“I am grieved that the cause of abolition should have so many excrescences”), the implications of living in a “hemmed in” condition went far beyond Hawaiian geography.³²

Though frequently immersed in the points of view and memories of the missionary women, Grimshaw does not forget the context of their presence in Hawaii. “With a self-confidence amounting to arrogance,” with “insensitivity and self-righteousness,” the “dedicated but painfully narrow” mission women set out to bring Hawaiian women, “no matter what the cost in suffering, into American ways.” Sisterhood ran aground on the mission “wives’ own cultural rigidity.”³³

The story of population decline that concerned David Stannard figures in this story as well. Whatever plans the missionaries had for the natives, disease and mortality cut them short. “Surely this people are melting away like dew”; “My soul is pained for this dying nation”; “What the Lord designs to do with this nation, his providence must disclose, but it sometimes seems to me as if he was about to sweep it away.” The mission women would not have to wait for demographers to call their attention to a disheartening pattern of population shrinkage.³⁴

“The children of the missionaries seem destined to possess the land,” wrote one woman in 1859.³⁵ Her observation calls attention to the outcome of the missionary enterprise that Grimshaw finds the most ironic of all. Despite the missionaries’ fear that their children would grow up strangers to American life, “their young had assimilated American culture so fully that they in turn helped conquer economically the Hawaiian society which their parents sought, once, to save spiritually.” If the success of missionaries in saving souls was mixed, the success of missionary children in acquiring land and power was not. As Grimshaw explains, “The missionaries’ Hawaiian proteges suffered, ironically, from the economic individualism to which missionaries themselves, in all good conscience, gave birth.”³⁶

With an array of extremely rich and resonant quotations from missionary women, *Paths of Duty* presents enormous possibilities for an exploration of perspective, consciousness, world-view, and *double entendre*, in the manner of Greg Denning. But, beyond the acknowledgement of Denning’s influence in the preface and the inclusion of several references to the missionary women’s inability to comprehend the “intricate, complex, and subtle . . . webs of meaning” that structured Hawaiian society, Grimshaw is not interested in the kind of intellectual adventure that draws Denning.³⁷ Grimshaw does note some remarkable moments of cross-cultural, mutual bewilderment. On one occasion, temporarily yielding to the campaign of the mission women on behalf of proper clothing, “some female chiefs of Honolulu squeezed themselves into corsets for church; desperately uncomfortable, they undressed themselves outside afterward and walked home,

³² Grimshaw, *Paths of Duty*, 127.

³³ Grimshaw, *Paths of Duty*, 195, 57, 195.

³⁴ Grimshaw, *Paths of Duty*, 178.

³⁵ Grimshaw, *Paths of Duty*, 188.

³⁶ Grimshaw, *Paths of Duty*, 193, 192.

³⁷ Grimshaw, *Paths of Duty*, xv.

quite unself-consciously, with their stays over their arms." What, one cannot help thinking, would Dening have made of that event? Or of the missionary wives' campaign to substitute cloth bonnets for the usual "wreaths of flowers worn round head and neck"?³⁸ What would Dening have made of the meanings attached to bonnets, to cloth, to flowers, and to the adornment of heads in these two reciprocally baffling cultures?

To thrive, Dening knows, scholars must be periodically productively unsettled and puzzled. When they take bonnets, corsets, and flowers for granted, when the world and its meanings begin to seem manageable, when the mysteries of human nature lose their power to bewilder, when causality becomes linear and clear, these are all the warning signals for the onset of intellectual complacency and stagnation. When these symptoms appear, Hawaiian history offers itself as a corrective. Hawaii is, to put it mildly, a very puzzling place. This may be especially true for Euro-Americans. In Hawaii, people of European descent are not the majority; "whites" or *haoles* are only one among several minorities; and monoculturalism, or any other alternative to multiculturalism, explains nothing about Hawaiian life, past or present. Eventually, in the shifting demography of the continental United States, whites will lose their majority status. The Hawaiianization of the nation is, in this sense, the wave of the future. This may be the Peripheral Studies Association's greatest prospective victory, a recognition that the center sometimes follows the edge, that American history survey courses as embarrassingly Hawaii-free as my own is at present are a disservice to young people who will live in a Hawaiianized future.

The transition from a historical profession intoxicated with the dream of objective, neutral, value-free inquiry to a historical profession aware of, and honest about, the unavoidable reality of subjectivity is, understandably, an awkward and uncomfortable one. "Be wary," Dening suggests, applying anthropological analysis to scholarly behavior, "of the history that claims to be separate from the circumstances of its telling or to have only one meaning."³⁹ Dening has clearly made his peace with this proposition and now lives comfortably, even pleasurably, in a professional world where subjectivity and uncertainty are fully acknowledged. Hawaiian history is, after all, contested property: commoners, chiefs, sailors, captains, missionaries, planters, laborers, tourists, resort developers, demographers, anthropologists, historians, women, men, Native Hawaiians, Euro-Americans, and Asian Americans all have their claims on the past. Here, then, is a focused case study in the disputes and debates over unity and diversity in national history. And here, too, in the example of Greg Dening, is a virtual prescription for making one's peace with life in the midst of contested turf. That peace rests on the propositions that uncontested turf would be too dull to bother with and that, for the historian in the late twentieth century, the unsettled life is the only life worth living. For those susceptible to a too-settled definition of the historian's task, Hawaiian history, with its virtually unlimited capacity to unsettle, offers the antidote.

³⁸ Grimshaw, *Paths of Duty*, 165, 164.

³⁹ Dening, *History's Anthropology*, 15.

Review Article

The Grass-Roots Right: New Histories of
U.S. Conservatism in the Twentieth Century

MICHAEL KAZIN

Leonard J. Moore, **Citizen Klansmen: The Ku Klux Klan in Indiana, 1921–1928**. Chapel Hill: University of North Carolina Press, 1991. 249 pp. \$29.95.

Kathleen M. Blee, **Women of the Klan: Racism and Gender in the 1920s**. Berkeley: University of California Press, 1991. 228 pp.

Jerome L. Himmelstein, **To the Right: The Transformation of American Conservatism**. Berkeley: University of California Press, 1990. 290 pp. \$25.00.

Ronald P. Formisano, **Boston against Busing: Race, Class, and Ethnicity in the 1960s and 1970s**. Chapel Hill: University of North Carolina Press, 1991. 323 pp. \$34.95.

Rebecca E. Klatch, **Women of the New Right**. Philadelphia: Temple University Press, 1987. 247 pp.

HISTORIANS, LIKE MOST PEOPLE, ARE RELUCTANT TO SYMPATHIZE with people whose political opinions they detest. Overwhelmingly cosmopolitan in their cultural tastes and liberal or radical in their politics, scholars of modern America have largely eschewed research projects about past movements that seem to them either bastions of a crumbling status quo or the domain of puritanical, pathological yahoos. Thus, over a decade after Ronald Reagan captured the presidency with the active support of the New Right, historians of the twentieth-century United States are just beginning to examine the origins of the type of popular conservatism whose standard the disarmingly sanguine president waved so successfully.¹

Until the recent appearance of studies that seek a new framework for understanding the twentieth-century Right, the dominant paradigm remained one established almost four decades ago. In 1955, a notable band of liberal social thinkers, horrified by the political mayhem recently committed by Senator Joseph

For advice and encouragement, I would like to thank Gary Gerstle, Beth Horowitz, John B. Judis, and Tony Fels.

¹ For a brilliant statement of this neglect and some suggestions on how to correct it, see Alan Brinkley, "The Problem of American Conservatism," unpublished lecture, in author's possession.

McCarthy and his ilk, contributed essays to *The New American Right*, an anthology edited by Daniel Bell. Characteristically, it was Richard Hofstadter (a superb stylist and the only author in the group trained as a historian) who best articulated the central theme that each essayist developed in diverse ways. "Pseudo-conservatism is in good part a product of the rootlessness and heterogeneity of American life," wrote Hofstadter, "and above all, of its peculiar scramble for status and its peculiar search for identity." "Status anxiety" came to the fore, he argued, when the nation was prosperous. In such periods, members of social groups alarmed at their declining position (especially Anglo-Saxons) and others on the rise (especially certain immigrants) feared conspiracies by shadowy elites to seize their property and corrupt their morals. Major conflicts of both the 1920s and the early 1950s fit this model; the Ku Klux Klan and the postwar Red hunters were its most prominent examples. The concept leaned "heavily," Hofstadter admitted, on Theodore Adorno's theory of the "authoritarian personality." The very term "pseudo-conservatism" (also borrowed from Adorno) signified the thoughtless, irrational nature of the subject. Peering through the reproachful lens of social psychology, Hofstadter and his fellow authors beheld the modern Right as a looming "populist" danger to civility and intellectual freedom rather than as a full-fledged alternative, at least potentially, to the reigning New Deal order.²

For over a decade after publication of the Bell anthology, intellectual discourse about the Right tended to fix on the notion of "extremist" or "radical" factions that had little in common with "true" conservatives, who were men of ideas, "temperate and compromising."³ Widely read books with titles like *Danger on the Right* and *The Politics of Unreason* and Hofstadter's famous essay on "the paranoid style," although they contain valuable narratives and organizational details, were transparent attempts to exclude a poisonous agent from the bloodstream of the body politic. They emphasized the violent language, prejudiced behavior, and willful marginality of groups ranging from embattled Federalist pamphleteers to the John Birch Society. This was a sociology and a history written by uneasy centrists. The horrors of fascism and Stalinism had taught such intellectuals, according to Seymour Martin Lipset and Earl Raab, to reject the siren call of "true believers" who "are too sure of what is right to be trusted with power in a complex world in which it is almost impossible to anticipate all the major consequences of social action."⁴

² As Hofstadter concluded his essay, "in a populist culture like ours, which seems to lack a responsible elite with political and moral autonomy, and in which it is possible to exploit the wildest currents of public sentiment for private purposes, it is at least conceivable that a highly organized, vocal, active and well-financed minority could create a political climate in which the rational pursuit of our well-being and safety would become impossible"; Richard Hofstadter, "The Pseudo-Conservative Revolt—1955," in *The Radical Right (The New American Right)*, expanded and updated, Daniel Bell, ed. (New York, 1962), 69–72, 79. The other contributors to Bell's original anthology were David Riesman, Nathan Glazer, Peter Viereck, Talcott Parsons, and Seymour Martin Lipset.

³ Hofstadter, "Pseudo-Conservative Revolt," 64. In a book published the same year as *The New American Right*, political scientist Clinton Rossiter identified the conservative tradition almost solely with intellectuals and national politicians. Rossiter, *Conservatism in America* (New York, 1955). Indicating the apparent hegemony of liberalism, Rossiter subtitled a revised edition of his book (published in 1962), *The Thankless Persuasion*.

⁴ Seymour Martin Lipset and Earl Raab, *The Politics of Unreason: Right-Wing Extremism in America, 1790–1977*, 2d edn. (Chicago, 1978), xviii. Arnold Foster and Benjamin R. Epstein, *Danger on the*

With the collapse of the liberal center in the late 1960s and early 1970s came a new intellectual fascination with ideological zealots and the movements they had built. Radical scholars, inspired by the work of British Marxists such as E. P. Thompson and Eric Hobsbawm, rediscovered the salience of ideological conflict and sharp social divisions in U.S. history. They created a new past filled with slaves, wage earners, farmers, and middle-class reformers who defended the republican creed against employers and government officials whose wealth and abusive power had betrayed it.⁵ At the same time, political sociologists, dissatisfied with the traditional model of anomic individuals buffeted by a maelstrom of change, developed new theories of “resource mobilization” that focused on how social movements defined and deployed their assets—media, staff, finances, appeals to moral conscience—for rational ends.⁶

But neither of these vigorous, innovative methodologies was initially applied to common Americans whose political passions usually diverged from those of liberalism and the Left, broadly defined (a shared dislike of “big business” notwithstanding). Thus the older Left’s erroneous belief that its conservative opposition was motivated by nothing but a defense of privilege and power went essentially unchallenged. Where they were not dismissed as a band of crackpots, movements on the right were deemed all but irrelevant to the history of American politics in the twentieth century. The only conflict that counted, it seemed, was the one between corporate liberals and the grass-roots champions of social equality and economic democracy.

By the early 1980s, a few young historians had begun to chip away at this pleasingly romantic formulation. Insurgencies of ordinary Americans, they suggested, were just as likely to fuel conservative causes as radical ones. Most significant in the revisionist enterprise were books by Alan Brinkley and Leo Ribuffo that focused on the 1930s. In *Voices of Protest: Huey Long, Father Coughlin, and the Great Depression*, Brinkley gracefully narrated the careers and analyzed the appeal of two major figures whom earlier scholars had tended to brush off as right-wing demagogues or even incipient fascists. As his title suggests, Brinkley rejected those pejorative assumptions. The “Kingfish” and “the Radio Priest” are better understood, he argued, as attractive, if unsystematic, spokesmen for a critique and vision—which Brinkley called populist—that many suffering Americans shared, whatever their electoral loyalties. When Long and Coughlin accused both “international bankers” and the architects of the New Deal of exacerbating the nation’s misery, they were voicing an “opposition to centralized authority,” a hostility to “the idea that human progress rested on continuing economic growth and organization,” and a “veneration of community institutions”—all of which people reared on Jeffersonian ideals held dear. This was a far cry from the faith

Right (New York, 1964); Richard Hofstadter, “The Paranoid Style in American Politics,” in *The Paranoid Style in American Politics and Other Essays* (New York, 1967), 3–40. Both the Lipset and Raab and the Foster and Epstein books were funded by the Anti-Defamation League of B’nai B’rith and thus appropriately attacked ethnic, racial, and religious prejudice.

⁵ An excellent survey of this scholarship is *The New American History*, edited for the AHA by Eric Foner (Philadelphia, 1990).

⁶ A useful summary is J. Craig Jenkins, “Resource Mobilization and the Study of Social Movements,” *Annual Review of Sociology*, 9 (1983): 527–53.

of Mussolini and Hitler, although Coughlin, when his popularity declined in the late 1930s, did stoop to reprinting the spurious, anti-Semitic *Protocols of the Elders of Zion*. In effect, Brinkley, without admiring them, had “rescued” two much-reviled figures and their followers “from the enormous condescension of posterity,” just as E. P. Thompson had advocated in his oft-quoted preface to *The Making of the English Working Class*. Their personal flamboyance and meteoric careers should not obscure the enduring traditions to which they appealed.⁷

Ribuffo had a more difficult task. His book, *The Old Christian Right: The Protestant Far Right from the Great Depression to the Cold War*, examined three leaders, each of whom made a chiliastic anti-Semitism central to his message. William Dudley Pelley, Gerald B. Winrod, and Gerald L. K. Smith never boasted the large followings of Long or Coughlin, and their fondness for wild accusations of conspiracies by Jews and other supposed elites had always made it easy for scholars to belittle them as men suffering from “paranoia, megalomania, aggression, and personal disorganization.”⁸

Ribuffo, while not minimizing the noxiousness of his trio, made an insightful point about them. All three men emulated fathers who had been both fundamentalist Protestant ministers and independent small businessmen; along with millions of citizens from similar roots, they cherished the values of “piety, propriety, upward mobility, deferred gratification, and hard work.” So Pelley, Winrod, and Smith were not alone in fearing the impact of a secularized culture and government officials who brought down Prohibition and curried favor with radicals and immigrants. They failed as political leaders because their clumsy publications and occasional election campaigns had nothing to offer but fear itself—plus a few sparks of reflected charisma. But, as Ribuffo pointed out, Jerry Falwell and other latter-day defenders of a putative “Moral Majority” learned from the mistakes of their benighted predecessors. Shorn, at least publicly, of anti-Semitism and anti-black racism and tolerant of moderate drinking, the New Christian Right proved a critical element in the victorious Republican coalition of the 1980s. This occurrence took academic historians by surprise, as it did many liberal Democratic politicians. As Ribuffo commented, “Unfortunately, most scholars emphasizing conflict in American history share a signal flaw with the consensus scholars whom they criticize: a hesitancy to take seriously groups that retard ‘progress.’”⁹

⁷ Alan Brinkley, *Voices of Protest: Huey Long, Father Coughlin, and the Great Depression* (New York, 1982), 161, 157, 159, and *passim*; E. P. Thompson, *The Making of the English Working Class* (New York, 1963), 12. Earlier, Ronald Radosh argued for the perspicacity of several “isolationist” writers and political figures of the 1930s and 1940s; Radosh, *Prophets on the Right: Profiles of Conservative Critics of American Globalism* (New York, 1975).

For an insightful study of how the ideology of the original Populists could serve both left and right-wing purposes, see Bruce Palmer, “Man over Money”: *The Southern Critique of American Capitalism* (Chapel Hill, N.C., 1980).

⁸ Lipset and Raab, *Politics of Unreason*, 167. This is the summary of an article written in 1951 by the sociologist Morris Janowitz.

⁹ Leo P. Ribuffo, *The Old Christian Right: The Protestant Far Right from the Great Depression to the Cold War* (Philadelphia, 1983), xvii, 258–74, xviii.

THAT PARTICULAR MYOPIA IS CONSPICUOUSLY ABSENT from the five historical works under review. Not coincidentally, all were written and published after the ascendancy of the Republican Right. Their authors—none of whom seem to be conservatives themselves—are determined to depict their subjects with the same sensitivity to cultural fabric and community context that mark the studies of republican artisans and assertive wage-earning women that continue to roll off academic presses.¹⁰ In fact, these studies of the grass-roots Right uncover some surprising parallels with movements usually associated with the Left, especially Populism and feminism. As is typical of self-conscious revisionists, the new monographers of the Right sometimes claim more influence and legitimacy for the particular movement under examination than seems justified by the evidence. And their focus on the identity and ideology of members slights questions of historical continuity and national influence, the very impact of these movements on large-scale political change. But the spotlight on conservative activists as purposeful, flexible, and idealistic protagonists in the public dramas of their day is a much-needed corrective to the “extremist” paradigm that still frames many discussions of the topic.

Contemporary scholars of the 1920s Ku Klux Klan (KKK) are the most assiduous exemplars of the new departure.¹¹ Over the past decade, detailed studies of Klan power in such surprising locales as El Paso and Anaheim have appeared together with articles and books about Indiana, Oregon, and Ohio, where the Invisible Empire was long known to be a major force. The principal argument of this unheralded output is a straightforward one: the KKK of the 1920s was not the bastion of displaced, provincial failures portrayed by most observers and scholars since H. L. Mencken. It appealed to the normative beliefs of ordinary, native-born white Protestants and primarily sought to exert those beliefs within its own ethno-religious community. The Klan’s racist, nativist, and often violent rhetoric was a poor guide to its behavior; KKK members primarily engaged in scrupulously legal battles against local politicians and businessmen—the great majority also being native-born Protestants—who were lax about enforcing Prohibition and financing more and better public schools. As Leonard J. Moore asserts in a useful review essay, “the Klan became a means through which average citizens could resist elite domination and attempt to make local and even state governments more responsive to popular interests.”¹²

Moore offers “populist” as a suggestive, if imprecise, name for the dominant impulse that drove well over two million men and women to join a group widely

¹⁰ Recent titles include Grace Palladino, *Another Civil War: Labor, Capital, and the State in the Anthracite Regions of Pennsylvania, 1840–68* (Urbana, Ill., 1990); Dorothy Sue Cobble, *Dishing It Out: Waitresses and Their Unions in the Twentieth Century* (Urbana, 1991); Mary Blewett, *We Will Rise in Our Might: Workingwomen’s Voices from Nineteenth-Century New England* (Ithaca, N.Y., 1991).

¹¹ This Klan is sometimes called “the second Klan” to differentiate it from the first, which briefly flourished during Reconstruction in the South from 1866 to the early 1970s, and the contemporary KKK, which arose during the 1950s to challenge the civil rights movement. For a comprehensive narrative, see David M. Chalmers, *Hooded Americanism* (New York, 1981).

¹² Leonard J. Moore, “Historical Interpretations of the 1920s Klan: The Traditional View and the Populist Revision,” *Journal of Social History*, 24 (Winter 1990): 353. For a representative anthology of revisionist essays (including Moore’s), see *The Invisible Empire in the West: Toward a New Historical Appraisal of the Ku Klux Klan of the 1920s*, Shawn Lay, ed. (Urbana, Ill., 1991).

associated, both then and now, with hatred and terrorism. This use reverses the pejorative connotation affixed to the term by Hofstadter and his fellow authors in *The New American Right*. As in Brinkley's book and the enormously influential writings of political consultant Kevin Phillips, "populism" evokes the rage and hopes of self-described "ordinary" or "average" men and women who rallied to defend themselves against a perceived attack by an establishment with immense powers at its disposal. Unmoored from its agrarian origins, the term "populism" appears in many accounts of modern right-wing movements. Although its meaning is as slippery and contested as "republican" or "liberal," "populist" is a handy way of signaling that the people being studied had sensible grievances that they pursued in a legitimate fashion. It tends to confer dignity and to repel both alarm and dismissal.¹³

To document this normalizing turn, Moore, in *Citizen Klansmen*, focuses on Indiana, where the Klan boasted more members per capita and more political influence than in any other state. At some time during the 1920s, up to one-third of all native-born white male Hoosiers were dues-paying members, and Klan activists dominated the Republican Party in most towns and cities and swept their candidates to victory in the state election of 1924. The genesis of this power, Moore maintains, lay in the ability of KKK organizers to portray themselves as the perpetuators of reform causes earlier associated with the People's Party and progressives. When they flailed "corrupt politicians" for illegally allowing the sale and transport of liquor, they were defending a crusade that had long been central to the evangelical moral fervor that nurtured the Social Gospel. When they alleged that the influence of parochial schools was growing, it was to draw attention to an alarming illiteracy rate and a public system that failed to provide immigrants and natives alike with schools "pulsating with the spirit of America," as one Klan leader put it. Moore points out that the KKK, in league with other school reformers, strongly endorsed the creation of a federal Department of Education. Xenophobic talk of Rome's "pagan ideals," he argues, was nearly always coupled with fears "that the power of the 'average citizen' had been diminished."¹⁴

Moore devotes the core of his book to documenting that Indiana Klansmen (he refers to the affiliated Women's KKK only in passing) were truly average, by every demographic marker. Armed with a freshly discovered 1925 membership list that includes virtually every one of the state's ninety-two counties, he convincingly

¹³ Moore, "Historical Interpretations," 353; Kevin P. Phillips, *Post-Conservative America: People, Politics, and Ideology in a Time of Crisis* (New York, 1982), esp. 31–52; Jonathan Rieder, *Canarsie: The Jews and Italians of Brooklyn against Liberalism* (Cambridge, Mass., 1985); Christopher Lasch, *The True and Only Heaven: Progress and Its Critics* (New York, 1991), 476–532. For an early critique of the ubiquity of the term in journalistic discourse, see George B. Tindall, "Populism: A Semantic Identity Crisis," *Virginia Quarterly Review*, 48 (Fall 1972): 501–18. Valuable discussions of the concept in an international perspective are Margaret Canovan, *Populism* (New York, 1981); and *Populism*, Ghita Ionescu and Ernest Gellner, eds. (London, 1969).

¹⁴ Leonard J. Moore, *Citizen Klansmen: The Ku Klux Klan in Indiana, 1921–1928* (Chapel Hill, N.C., 1991), 7, 37, 19, 23. Moore also notes that Indiana Klan leader D. C. Stephenson had been "a socialist in Oklahoma before World War I": p. 14. For other fragmentary evidence of former socialists who became Klansmen, see Donald T. Critchlow, ed., *Socialists in the Heartland: The Midwestern Experience, 1900–1925* (South Bend, Ind., 1986), 13–14; Richard W. Judd, *Socialist Cities: Municipal Politics and the Grass Roots of American Socialism* (Albany, N.Y., 1989), 180–82.

demonstrates that the organization was rejected only by those male Protestants who were very rich, very poor, or who denied the beneficence of the “noble experiment” (Herbert Hoover’s term for Prohibition). Cities and rural areas were equally represented; the Klan was more popular where large, commercial farms or booming factories dominated the economy than in languishing regions; and renters were more likely to join than homeowners—although the \$10 initiation fee, monthly dues, and cost of the infamous robes clearly dissuaded the most indigent Hoosiers. Moore also found, to the surprise of those not familiar with the history of American religion in the 1920s, that members of fundamentalist churches were markedly underrepresented in the KKK, although their pastors voiced the same kind of cultural jeremiads as did speakers for the Klan. In fact, Quakers, adherents of a denomination that had traditionally supported moralist causes, joined the Hooded Order far more readily than did Christians, whose pastors were advising them to shun the “worldliness” that a controversial secret society typified.¹⁵

This accumulation of data, crisply and cogently presented, is the fullest, most impressive statement of the new Klan scholarship. Together with Moore’s outline of the organizational ideology of “white Protestant nationalism,” it helps clarify why the KKK became a lightning rod for both supporters and opponents in the 1920s. No mere vehicle of marginal malcontents could have blossomed almost overnight into a powerful machine capable of carrying a hotly contested state election and dividing a national party convention in two (as the Klan did to the Democrats in 1924).

But Moore’s stress on the breadth and legitimacy of the Hooded Order fails to explain the political fate of this explicitly political organization. As he demonstrates, the Indiana KKK did not start to grow until mid-1922, and it was already declining, never to rebound, by the time the statewide membership list was compiled three years later. Many local branches, or klaverns, in other parts of the nation had an even shorter life.¹⁶ Moore and his fellow revisionists blame the collapse mostly on internal divisions and the indictments of leaders for a range of crimes, all well-publicized by a hostile press. In Indiana, the arrest and conviction of Grand Dragon D. C. Stephenson for the grisly rape and mutilation of twenty-eight-year-old Madge Oberholtzer certainly provided an excellent reason to desert an order supposedly dedicated to protecting Christian womanhood. But other mass movements on both the right and left have weathered the fall of hypocritical or opportunistic leaders without folding. Perhaps the Klan, whose initial national growth spurt was the work of clever publicists, clung too close to the social doctrine of native-born Protestant pietists. Local ministers and Republican politicians often voiced the same concerns about education, the liquor trade,

¹⁵ Moore, *Citizen Klansmen*, 7–8, 43. For the statistical analysis, see 44–75; for a discussion of conditions in three communities where the Klan was strong, see 107–50. Moore notes that the unpopularity of fundamentalism in the state yielded a sample “too small to be analyzed by the regression technique”; p. 75.

¹⁶ For example, in the small city of La Grande, Oregon (population 7,000), a Klan chapter was founded in May 1922, rapidly became a powerful pressure group of almost 400 members, and was moribund by the end of 1923. David A. Horowitz, “Order, Solidarity and Vigilance: The Ku Klux Klan in La Grande, Oregon,” in Lay, *Invisible Empire*, 185–215.

and new immigrants; they lacked the collective intensity of the Klan, but their legitimacy was never in question. When the KKK's notoriety outshone its expressive value, there was no good reason to stay on board. Moore's rather thin, repetitive descriptions of the Klan's "populism" and "traditional beliefs" cannot account for why it fell as quickly as it rose.¹⁷

A delineation of what those beliefs meant to both sexes in the Invisible Empire is needed to understand its growth. Kathleen Blee's study of the Women of the Ku Klux Klan (WKKK) concentrates on the aspects of Klan life—primarily in Indiana but also in the nation—that Moore's statistical analysis and electoral narrative neglect: gendered symbols and behavior and the building of a political culture. *Women of the Klan* is already gaining wide attention because it effectively disputes the largely unexamined notion that female activists who challenged male domination nearly always belonged to a left-wing tradition of social equality and ethnic tolerance. The WKKK had chapters in over two-thirds of the states, and, while reliable totals are unavailable, its membership may have rivaled that of its male counterpart. It clearly encompassed thousands of women who viewed the organization as furthering the same reforms—Prohibition, woman suffrage, and a moral housecleaning of public life—that most self-described "progressives" had favored before World War I. Klanswomen saw their hostility toward other ethnic and religious groups as a necessary means of self-defense against a decadent, anti-maternal threat. Thus members of the WKKK, under the sometimes uneasy supervision of the all-male parent body, according to Blee, articulated "a language of rights, arguing that vice and immorality denigrated white Protestant women and that [they] had the right, and the duty, to end it."¹⁸

The approach of the WKKK was overwhelmingly informal, personal, and neighborhood centered—elements shared by female activists at the time, whatever their politics. Klanswomen spearheaded "poison squads" that encouraged consumers to boycott retail businesses owned by Catholics and Jews. They sponsored children's skits and sing-alongs featuring new verses to old tunes; titles included "Yes, the Klan Has No Catholics" and "Onward Christian Klansmen." They brought to Klan picnics and parades the same skills for cooking, decoration,

¹⁷ Moore, *Citizen Klansmen*, 101, 103. The second Ku Klux Klan was founded in 1915 by Atlanta businessman William J. Simmons. But it was Edward Young Clarke and Elizabeth Tyler, veteran practitioners of public relations who had worked for the Red Cross and the Anti-Saloon League, who managed its expansion. Their main motivation was a healthy slice of the \$10 each initiate had to pay. See the summary in Kathleen M. Blee, *Women of the Klan: Racism and Gender in the 1920s* (Berkeley, Calif., 1991), 17–23. For an older explanation of the Klan's decline that focuses on "its lack of a positive program and a corresponding reliance upon emotion rather than reason," see Kenneth T. Jackson, *The Ku Klux Klan in the City, 1915–1930* (New York, 1967), 251–55 (quote 255).

¹⁸ Blee, *Women of the Klan*, 71; on the size of the WKKK nationally, see 29–30. In Indiana, Blee claims a female membership comparable to that Moore computed for men, "a quarter-million during the 1920s, or 32 percent of the entire white native-born female population of the state"; *Women of the Klan*, 125. On the tangled relationship between the men's and women's Klans, see 42–69.

Other works that challenge the exclusively leftist orientation of the discourse of woman's rights in the twentieth century include Aileen Kraditor, *The Ideas of the Woman Suffrage Movement* (New York, 1965); Leila Rupp and Verta Taylor, *Survival in the Doldrums: The American Women's Rights Movement, 1945 to the 1960s* (New York, 1987); and Rebecca E. Klatch, *Women of the New Right* (Philadelphia, 1987) (see discussion below). A fascinating study of the immediate pre-history of the Klan that focuses on changing images of women is Nancy MacLean, "The Leo Frank Case Reconsidered: Gender and Sexual Politics in the Making of Reactionary Populism," *Journal of American History*, 78 (December 1991): 917–48.

and self-display honed at years of church functions and holiday gatherings both secular and spiritual. The religious connection was a natural one. "In many communities in Indiana," writes Blee, "Klan events were difficult to distinguish—except for their excitement—from those of Protestant congregations . . . Despite its trappings of secrecy, the Klan was commonplace, an accepted part of life in native-born white Protestant families."¹⁹

Blee thus agrees with Moore's depiction of Klan normality and legitimacy, although she chooses a different route to get there. Individual activists, a handful of whom she profiles with welcome detail, cared passionately about ending the "traffic" in both liquor and young women. The WKKK provided at least a temporary means of promoting these popular causes within a semi-autonomous, collective structure that required women to be vocal and resourceful. There were, in other words, rational, even liberatory reasons for women to join the Klan.²⁰

Unfortunately, the richness of Blee's ethnographical treatment gives little hint of why women soured on the Klan as quickly and decisively as their men did. Blee seems content to illuminate a vanished world and to leave the manner and meaning of its passing for others to explore. But the Klan, like any serious political organization, sought power. And a reputation for extremism and violence, whether or not it was justified, certainly allowed the Klan's many enemies to speed its downfall. What may have appeared "commonplace" and "traditional" to Klan members and sympathizers was, to many more Americans (especially recent immigrants and their children who inhabited the largest cities) a deadly menace. The KKK's initial success and its subsequent failure should be examined together, the consequence of a rhetorical strategy that demonized

¹⁹ Blee, 167, 169; for the details of Klan political culture, see 123–73. On the style of female activists, see Paula Baker, *The Moral Frameworks of Public Life: Gender, Politics, and the State in Rural New York, 1870–1930* (New York, 1991). The view of the Klan as normal is not altogether new. In 1923, H. L. Mencken and George Jean Nathan argued, in *Smart Set*, that the Hooded Order shared the same beliefs as the mainstream society they memorably lampooned: "If the Klan is against the Jews, so are half of the good hotels of the Republic and three-quarters of the good clubs . . . If the Klan is for damnation and persecution, so is the Methodist Church. If the Klan is bent upon political control, so are the American Legion and Tammany Hall . . . If the Klan constitutes itself a censor of private morals, so does the Congress of the United States"; quoted in Chalmers, *Hooded Americanism*, 1.

²⁰ For example, before the Klan had been reestablished, Daisy Barr (born in 1876) was already a Quaker minister, a leading activist in the Indiana prohibition movement, an organizer of the Muncie YWCA, and the creator of a refuge for prostitutes. She took her formidable talents into the WKKK because she viewed the Klan as the only serious group combating the scourges of moral womanhood and because she shared its nativist views. Blee, *Women of the Klan*, 104–11.

To her credit, Blee does mention a kooky, paranoid side of the Klan that Hofstadter and David Brion Davis long ago recognized as endemic to "counter-subversive" movements. According to a former WKKK member she interviewed, one Indiana leader hinted that "the Pope was coming to take over the country, and he said he might be on the next train that went through North Manchester. You know, just trying to make it specific. So, about a thousand people went out to the train station and stopped the train. It only had one passenger train and one passenger on it. They took him off, and he finally convinced them that he wasn't the Pope. He was a carpet salesman. And so they put him on the next train and he went on to Chicago"; Blee, *Women of the Klan*, 149; Hofstadter, "Paranoid Style"; David Brion Davis, ed., *The Fear of Conspiracy: Images of Un-American Subversion from the Revolution to the Present* (Ithaca, N.Y., 1971). The classic account of the Muncie KKK as a deluded, divisive force is Robert S. Lynd and Helen Merrill Lynd, *Middletown: A Study in Modern American Culture* (New York, 1929), 481–84.

millions of urban dwellers who were just beginning to perceive themselves as political subjects.²¹

To adequately understand the short, turbulent life of the Klan, a larger framework is necessary. The Invisible Empire was only the most controversial of a number of mass organizations that arose during and just after World War I to combat “un-American” radicalism in its varied but often complementary forms: pacifism, syndicalism, bolshevism, artistic and sexual modernism, racial integration, anti-war feminism, and “progressive” (that is, Deweyite) education. In fact, the existence of an entity large and coherent enough to be termed a “movement” of the Right probably required the simultaneous impetus of the galvanic, left-wing upsurge that shook up universities, workplaces, and governments all across Europe and North America in the years from 1917 to the early 1920s.²²

The American Legion was the largest and best organized of the anti-Left groups, but its brand of respectable outrage both depended on and helped inspire similar efforts dedicated to casting out the traitor and the libertine while exalting communities of the self-disciplined, the devout, and the patriotic. For activists whose prime motivation was religious (which included few Legionnaires), support for Prohibition seems to have been a defining position. The Anti-Saloon League and Women’s Christian Temperance Union had already persuaded hundreds of thousands of Protestant activists to view the scourge of liquor as a powerful symbol of all that was wrong in America. So, in the 1920s, anger at public officials who disregarded a measure that represented the fruit of decades of struggle fused with resentment against the cosmopolitan, the immigrant, and the radical—all of whom mocked or flaunted the Volstead Act. Long ago, John Higham, in his landmark study of nativism, linked these phenomena together as “The Tribal Twenties.”²³ But no one has yet studied their nature as intersecting social movements, ones that received intermittent encouragement from agencies of the state (especially the new FBI) and other authorities.

²¹ On the political preferences of Catholics and immigrants in the 1920s, see Allan J. Lichtman, *Prejudice and the Old Politics: The Presidential Election of 1928* (Chapel Hill, N.C., 1979).

²² In 1924, one section of the War Department constructed a spider-web chart connecting a number of pacifist, socialist, and feminist associations. See Nancy Cott, *The Grounding of Modern Feminism* (New Haven, Conn., 1987), 242, 249. On the labor aspects of the upsurge, see the collected essays in James Cronin and Carmen Sirianni, eds., *Work, Community, Power: The Experience of Labor in Europe and America* (Philadelphia, 1983). From the Civil War to World War I, conservatism in America was predominately an anti-democratic ideology articulated by certain intellectuals, jurists, and industrialists. Still useful as a survey is Robert Green McCloskey, *American Conservatism in the Age of Enterprise* (Cambridge, Mass., 1951).

²³ John Higham, *Strangers in the Land: Patterns of American Nativism, 1860–1925* (New Brunswick, N.J., 1955), 264–99. For a provocative argument that the Klan was “the most visible and powerful guardian” of Victorian moral and cultural values and a succinct summary of the new scholarship, see Stanley Coben, *Rebellion against Victorianism: The Impetus for Cultural Change in 1920s America* (New York, 1991), 136–56. The American Legion continued to prosper long after the postwar era by combining anti-radicalism with skillful lobbying for veterans’ benefits and service activities such as disaster relief, education on the flag, and teenage baseball leagues. On the American Legion in this period, see William Pencak, *For God and Country: The American Legion, 1919–1941* (Boston, 1989). On the National Security League, a short-lived precursor to the Legion that boasted 85,000 members, see Michael Pearlman, *To Make Democracy Safe for America: Patricians and Preparedness in the Progressive Era* (Urbana, Ill., 1984), 161–62, 179–82.

FROM THE NEW HISTORIES of the 1920s Klan, there does emerge, in embryo at least, a new paradigm of grass-roots conservative ideology that roughly coincides with what scholars of more recent movements on the right have discovered. This paradigm stresses two major themes: first, the opposition between self-governing, pious communities and decadent, hypocritical, globe-trotting elites; second, the attempt to turn Americans away from consumerism and unearned welfare and toward a homogeneous morality of hard work, self-control, and self-reliance. It also includes, especially from the 1930s on, a hostility to international cooperation and most diplomatic agreements and the conviction that force is necessary to achieve "law and order" both at home and abroad. Notwithstanding their many differences, Klan leaders of the 1920s as well as Father Coughlin and Huey Long, Barry Goldwater and George Wallace, Phyllis Schlafly and David Duke all articulated these themes. Since the end of World War I, there has been an unbroken string of movements to carry on the struggle.²⁴

For most historians, however, the period from the late 1920s to the early 1960s is uncharted terrain. Even the excellent works by Ribuffo and Brinkley (and a number of fine, separate treatments of Long and Coughlin) examine only the most flamboyant, best publicized figures from the 1930s. Milo Reno's important Farm Holiday Association and the ideologically diverse movement against military intervention abroad (organized from 1939 to 1941 by America First) have yet to attract scholars concerned with social and cultural dynamics.²⁵ Moreover, for all the attention devoted to McCarthy and McCarthyism, there are almost no studies on the anti-Communist movement itself, ubiquitous in the United States during the late 1940s and 1950s. We know that millions of Americans marched in Loyalty Day demonstrations, prayed regularly for the "conversion" of Russia, idolized General MacArthur after President Truman took away his command, and participated in Red-hunting activities sponsored by groups like the American Legion and the John Birch Society. But we do not yet know, with any degree of precision, who these mobilizers and followers were and what normative beliefs they shared with earlier conservative activists.²⁶

Continuity is the principal motif of *To the Right*, Jerome L. Himmelstein's

²⁴ Some of these individuals and the movements they led, especially in the 1930s, wanted the state to help secure "social justice." Long's "Share the Wealth" plan and Coughlin's support for a guaranteed annual wage are good examples. But, unlike the Left of their day, both men opposed all types of public ownership and government jobs programs such as the Public Works Administration as threats to the Constitution. Brinkley, *Voices of Protest*, 153–56.

²⁵ On the isolationists, see Wayne S. Cole, *America First: The Battle against Intervention* (Madison, Wis., 1953); Justus D. Doenecke, *In Danger Undaunted: The Anti-Interventionist Movement of 1940–1941 as Revealed in the Papers of the America First Committee* (Stanford, Calif., 1990).

²⁶ However, for some clues about the John Birch Society and kindred "extremist" groups, see Lipset and Rabb, *Politics of Unreason*, 248–337. Several fine studies have been done of the intellectual Right in these years. The most complete is George H. Nash, *The Conservative Intellectual Movement in America since 1945* (New York, 1976). Also see John B. Judis, *William F. Buckley, Jr.: Patron Saint of the Conservatives* (New York, 1988); J. David Hoeveler, Jr., *Watch on the Right: Conservative Intellectuals in the Reagan Era* (Madison, Wis., 1991). The most comprehensive study of the Red scare is David Cauter, *The Great Fear* (New York, 1978). But Cauter—like most writers on the subject other than biographers of McCarthy, J. Edgar Hoover, and other famous individuals—focuses more on the damage being done than on those responsible for it. For a taxonomy of the anti-Communist Right in the mid-1950s, see Richard Gid Powers, "The Roy Cohn Dinner, July 28, 1954," paper given at convention of Organization of American Historians, Louisville, 1991 (in author's possession).

thoughtful synthesis on the rise of the contemporary Right. Unlike the new historians of the KKK, his primary concern is political success and failure—what amalgam of ideas and organization contributed to one outcome or the other. The conservatives who took national power in the 1980s, Himmelstein argues, were the spiritual descendants of such anti-statist critics of the New Deal as Herbert Hoover and Robert Taft. After World War II, they refashioned their ideology to take advantage of widespread anti-Communist fervor and a growing mistrust of liberal “bureaucrats.” Writers such as William F. Buckley, Jr., and Frank Meyer, whose primary organ was *The National Review*, embraced a militantly interventionist foreign policy and challenged federal officials to purge the government of all suspected Communists and their sympathizers. At the same time, they fused the libertarian creed of economists Friedrich Hayek and Milton Friedman with the hunger for an “objective moral order and virtue” that philosophical conservatives had always favored. Religious faith of an unaccustomed ecumenical kind became the linchpin connecting “the free market” and “traditional values,” the magic key to prosperity and the sturdy triad of family, church, and native place.²⁷

This doctrinal amalgam, argues Himmelstein, remained intact into the 1980s. President Reagan’s fondness for both *Human Events* and Buckley’s *National Review* (journals that began publishing in the mid-1940s and mid-1950s, respectively) symbolized the endurance and maturation of a single world-view. But to achieve electoral success, conservatives did have to alter their language and broaden their base. By the late 1970s, journalists began noticing a “New Right” that, remarkably, joined together businesspeople who chafed at federal regulations with former working-class Democrats outraged at cultural permissiveness and court-ordered busing. Both groups agreed that a “liberal establishment” was largely to blame for what ailed the nation.²⁸

Himmelstein, a sociologist, adapts a dualistic model drawn from resource mobilization theory to explain what happened. The mass Right, he maintains, was neither an outside “challenger” to centers of national power nor a member of the “polity” of insiders. “The great strength of the conservative movement,” he writes, “was that it had characteristics of both groups . . . [I]t combined many of the resources of a member with the capacity to talk like a challenger.”²⁹ Corporate executives and Republican campaign operatives contributed money, political skills, and the ability to use various media (especially direct mail) to quickly take advantage of the mistakes of their liberal opponents. The evangelical Right brought a mushrooming number of new constituents eager to battle the perceived allies of abortionists, drug pushers, and nuclear disarmers. In combination, these two forces—together with fears of economic decline that could plausibly be blamed on President Jimmy Carter, the Democratic incumbent—propelled into

²⁷ Jerome L. Himmelstein, *To the Right: The Transformation of American Conservatism* (Berkeley, Calif., 1990), 28–62 (quote, 57).

²⁸ Two excellent studies that document and analyze the institutional strength of the New Right, financed largely by corporations based in the West and Southwest, are Thomas Byrne Edsall, *The New Politics of Inequality* (New York, 1984); and Sidney Blumenthal, *The Rise of the Counter-Establishment: From Conservative Ideology to Political Power* (New York, 1986).

²⁹ Himmelstein, *To the Right*, 63–64.

office the most ideologically conservative administration since that of Calvin Coolidge.

Viewing the Right through the single lens of pragmatic coalition-building has its virtues. In particular, it recognizes that the making of a new electoral majority was a long-term project requiring the coordination of visionary activism and institutional support (as well as favorable demographic trends such as the growth of the Sunbelt). But Himmelstein unduly minimizes the importance of “populist” language in making victory possible and shaping its meaning. He thus disagrees with Kevin Phillips’s oft-articulated argument that the New Right, which began to organize in the late 1960s, represented a momentous break with an elitist conservatism unable to shed its association with the smug and wealthy. The attack on “the establishment and the elite in the name of the people,” writes Himmelstein, “was just rhetoric, and even as rhetoric it was hardly new.” The issues that united conservatives—militant anti-Communism, cuts in taxes and domestic spending, and the restoration of public morality—had not changed since the heyday of Joe McCarthy. Only the political mobilization of the mushrooming evangelical churches was novel.³⁰

“Just rhetoric,” however, can help bring about a sea change when it inspires and expresses the discontent of a pivotal constituency. Himmelstein correctly notes that “social issues” did not elect Ronald Reagan president in 1980, although they did help him win the nomination over the then-moderate George Bush and Howard Baker. But, by that time, conservative politicians were already skilled at courting the many Democrats (of different religions or none at all) who resented the iconoclastic behavior of black radicals and white radicals and the liberals who appeared to encourage their rebellion. A decade earlier, Richard Nixon’s praise of “the silent majority” and “Middle America” made a smooth, upbeat complement to George Wallace’s bitter jabs at “student anarchists” and “pointy-headed intellectuals.” During the 1970s, the national Republican Party learned, for the first time, how to harness the discourse of community, the producer ethic, and direct democracy that social movements on the left (and, ambiguously, a few on the right) had often employed. The very fact that one of his closest presidential advisers viewed Ronald Reagan as “a populist” and not as a traditional conservative Republican reveals the extent of the change. The new language of the Right certainly did not shape the policies of either the Nixon or Reagan administrations, most of which were resolutely pro-corporate. But it framed them as a vital corrective to the damage supposedly done by haughty liberals oblivious to the desires of the virtuous majority.³¹

³⁰ Himmelstein, *To the Right*, 8, 92. For Phillips’s evolving interpretation, see *The Emerging Republican Majority* (New Rochelle, N.Y., 1969); *Mediocracy* (New York, 1977); *Post-Conservative America*.

³¹ The adviser was Budget Director Richard Darman; Peggy Noonan, *What I Saw at the Revolution: A Political Life in the Reagan Era* (New York, 1990), 263–64. Noonan, an Irish Catholic from a working-class family of Democratic voters, skillfully emphasized sentimental populist themes in many of the speeches she wrote for the president. By the 1988 campaign, such rhetoric was being reduced to banal boilerplate. See, for example, the acceptance speech by vice-presidential nominee Dan Quayle, “Quayle’s Speech: Winning ‘One for America’s Future,’” *New York Times* (August 19, 1988): A17. On the development of conservative populism, see Jonathan Rieder, “The Rise of the ‘Silent

One of the key insurgencies that generated grass-roots resentment against a liberal elite was the anti-busing movement. *Boston against Busing*, Ronald Formisano's book about Boston's civic agony is a meticulous narrative about one of the largest, and certainly the best publicized, conflicts that pit white urban neighborhoods against an integration plan designed by a federal judge friendly to the civil rights movement.³² Behind the epithets, curses, and rock throwing that attended the implementation of Judge W. Arthur Garrity, Jr.'s decision in the fall of 1974, there lay a grand political irony. The white working-class protestors in Boston who charged "judicial tyranny" actually had most federal officials, especially elected ones, on their side. President Nixon and all but a handful of Republicans firmly opposed busing, and the Democratic-controlled House of Representatives had already passed a bill forbidding the use of federal money to buy gasoline to bus any student away from "the public school closest to his home." In Boston, where only 16 percent of the population was black, Mayor Kevin White could muster up only enough courage to make a series of weak appeals to obey the law. He muted talk of racial equality and even made a secret neutrality pact with Louise Day Hicks, popular leader of the anti-busing movement. As Thomas Edsall and Mary Edsall wrote, "A dwindling band of northern liberals found that they were defending a policy with no real constituency." As early as 1971, only 18 percent of the public, according to a Gallup poll, favored busing; even blacks opposed it, albeit by a very slight margin.³³

Anti-busing activists in Boston thus had to worry less about converting local politicians and their fellow citizens than about enforcing their will on a stiff-necked, unpopular authority. In this sense, the heavily Catholic membership of Restore Our Alienated Rights (ROAR)—spearhead of the resistance campaign—was in a position like that of the Indiana KKK a half-century before. Both organizations aggressively articulated the "common sense" of a large, if culturally bounded, constituency: that immoral, undemocratic forces of outsiders were preying on ordinary people with "traditional" values.³⁴ Both groups immediately drew the ire of urbane, left-leaning intellectuals and journalists. And both groups wielded a local political base—in ROAR's case, the elected Boston School Committee—to pressure its opponents to give up the fight.

The anti-busers, however, despite their violent image, were more fortunate than the Klan. They and their concerns were riding a cresting wave of anti-

Majority," in *The Rise and Fall of the New Deal Order*, Steve Fraser and Gary Gerstle, eds. (Princeton, N.J., 1989), 243–68.

³² Most readers of Formisano's book will be familiar with Anthony Lukas's splendid account of Boston before and during the busing crisis, a lengthy volume that won the Pulitzer Prize and other honors: *Common Ground: A Turbulent Decade in the Lives of Three American Families* (New York, 1985). Formisano does not have Lukas's storytelling talents, but he does provide a cogent political framework for the conflict and a valuable interpretation of the language of the anti-busing movement. His book thus complements the journalist's masterpiece.

³³ Thomas Byrne Edsall with Mary D. Edsall, *Chain Reaction: The Impact of Race, Rights, and Taxes on American Politics* (New York, 1991), 89–90.

³⁴ By "common sense," I mean the Gramscian term denoting a received "conception of the world which is uncritically absorbed by the various social and cultural environments in which the moral individuality of the average man is developed"; quoted in Walter L. Adamson, *Hegemony and Revolution: A Study of Antonio Gramsci's Political and Cultural Theory* (Berkeley, Calif., 1980), 123; for a discussion, see 149–52.

liberalism. The plight of “the white ethnic” caught between “bleeding heart” authorities and the black underclass was rather easily coopted by rising conservative politicians. But the KKK, notwithstanding its link to one strand of progressive thinking, stood for a nativist supremacy whose demographic and ideological foundations were rapidly eroding. Eight years after the apogee of the 1920s Klan, Franklin Roosevelt was elected president. Seven years after ROAR was organized, Ronald Reagan took the oath of office.

Still, as Formisano illustrates, the anti-busing cause was, at the time, an authentic protest movement that called on the dormant abilities of thousands of white Bostonians. Women and men who had previously been cynical about politics planned school boycotts and street demonstrations, gave speeches and wrote articles, learned the art of lobbying and campaigning. Living in an urban area where the New Left had recently thrived, the anti-busers ironically adopted many “anti-establishment” tropes and symbols. They chanted “Hell, no! We won’t go!,” cited Martin Luther King, Jr.’s defense of civil disobedience, marched on Washington, and proudly affixed handfuls of militant buttons to their dresses, windbreakers, and backpacks. Such anti-authoritarian flamboyance enthralled journalists whose appetite for right-wing “populism” had already been whetted by three George Wallace presidential campaigns and Vice-President Spiro Agnew’s stinging attacks on media executives as “a tiny and closed fraternity of privileged men.” The same media that ROAR activists denounced for supporting Judge Garrity and calling them racists were, in effect, publicizing their feisty rebellion to the world.³⁵

The anti-busers also enhanced their reputation with language reminiscent of other “forgotten” men and women, their loyalties fitting no received notion of Right or Left. “We’re the poor sunavabees who pay our taxes and sweat tuitions, sweat mortgages and car payments and the cost of groceries and fuel, get no hand outs, give our blood, take our turn in line, volunteer for charities, and work two jobs, sometimes three,” wrote Dick Sinnott, a local columnist whom Formisano calls “the most popular print spokesman of the antibusing legions.” Frank Capra had gained his reputation by putting celluloid flesh on descriptions like that; the Catholic ethic of selfless if burdensome service to neighborhood, family, and faith motivated both the pro-New Deal filmmaker and the backlash commentator. Shorn of its altruistic features, Sinnott’s self-description also fit a media personality of the time: the Archie Bunkers of Boston seemed to have thrown down their newspapers, jumped up from their easy chairs, and gone out to demonstrate for “community schools.”³⁶

Soon after the American Right did assume national power, however, its

³⁵ Ronald P. Formisano, *Boston against Busing: Race, Class, and Ethnicity in the 1960s and 1970s* (Chapel Hill, N.C., 1991), 138–46; Agnew quoted in Frederick F. Siegel, *Troubled Journey: From Pearl Harbor to Ronald Reagan* (New York, 1984), 229.

³⁶ Formisano, *Boston against Busing*, 187, 189. On the Catholic origins of Capra’s populist sensibility, see Lee Lourdeaux, *Italian and Irish Filmmakers in America: Ford, Capra, Coppola, and Scorsese* (Philadelphia, 1990), esp. 136–41. Even though Lourdeaux connects this to Italian Catholicism, I believe its main features were shared by other Catholics from working-class neighborhoods. The best study of media and modern social movements is Todd Gitlin, *The Whole World Is Watching: Mass Media in the Making and Unmaking of the New Left* (Berkeley, Calif., 1980). Comparable studies remain to be done on the post-World War II Right.

grass-roots image began to fade, and fissures began to show. This was, perhaps, an inevitable fate for a movement that had combined contradictory faiths without actually resolving their differences: communitarians mistrustful of mass culture with champions of an unrestricted free market; internationalists dreaming of a new American Century with skeptics whose foreign policy had always been America first. Over the course of the 1980s, quarrels between “paleo” and “neo” conservatives gradually moved out of right-wing journals and erupted onto the opinion pages of the daily *Washington Times*, the *Wall Street Journal*, and on television talk shows featuring such pugnacious pundits as Patrick Buchanan and Ben Wattenberg. In 1990, one of the best analysts of the contemporary Right observed, “There is no longer any set of fixed beliefs that characterizes conservatives.”³⁷

In the early 1980s, sociologist Rebecca Klatch had already glimpsed one source of this disharmony when she interviewed, at length, a sample of thirty female activists. *Women of the New Right* portrays two sharply different world-views among people who shared a hatred of Communism and “big government” and an admiration for the politicians of the Republican Right. “Social conservatives” cared most deeply about the supposed breakdown of “family values” represented by legalized abortion, high divorce rates, the tolerance of homosexuality, and “self-centered” feminism. They resented the state for interfering in the familial and local sphere by, for example, distributing birth control information to teenagers and banning prayer in public schools. In contrast, “laissez-faire conservatives” were women who perceived all political issues through the lens of individual liberty. They endorsed the goal of “equal rights” but objected to the feminist movement’s fondness for statist remedies to women’s problems (through such instruments as the Equal Rights Amendment and the funding of child-care centers). Klatch demonstrates how tentative was the bond between those activists who lauded the nurturing skills of pious, contented housewives and those whose heroines were Ayn Rand and Jeanne Kirkpatrick. She concludes, “the New Right speaks a common language devoid of any common meaning.”³⁸

However, the division Klatch identifies is more meaningful as doctrine than as politics. On the one hand, right-wing libertarianism has never been a large-scale movement; while Americans grumble constantly about state “interference,” only small minorities, at least in the twentieth century, have rallied to candidates who promise them a markedly less active government or who take a neutral stance toward cultural issues. In the 1980s, apart from a few Washington think tanks, there were no “laissez-faire” organizations on the right of any size or power.³⁹

³⁷ John B. Judis, “The Conservative Crackup,” *The American Prospect*, 3 (Fall 1990): 41. Also see Judis, “Conservatism and the Price of Success,” in *The Reagan Legacy*, Sidney Blumenthal and Thomas Byrne Edsall, eds. (New York, 1988), 135–71.

³⁸ Klatch, *Women of the New Right*, 101, 127–29, 148–50, 196. Most of the interviews were conducted in Massachusetts while Klatch was a graduate student at Harvard; thus her sample includes a disproportionate number of Catholics. A summary and updating of her analysis appears as Klatch, “Complexities of Conservatism: How Right-Wing Women Understand the World,” in *America at Century’s End*, Alan Wolfe, ed. (Berkeley, Calif., 1991), 325–39.

³⁹ The Libertarian Party, from which Klatch takes a few of her informants, should not be placed on the right. Committed to the principle of a minimal state, it opposed the military buildup of the early 1980s and supported the unrestricted right to abortion and the legalization of drugs. Its 1980

On the other hand, "social conservatives" retain a sizable base among blue-collar Catholics and in many evangelical churches. Their influence in the right-to-life movement, in grass-roots campaigns against pornography, and in many local Republican organizations is considerable, even though their hopes of capturing national office with a standard-bearer like Pat Robertson or Jesse Helms are gone. What is left of the New Right probably ensures that the "culture wars" of the 1960s will rage until the end of this century and beyond. Like the anti-modernist Klan of the 1920s, the contemporary traditionalist Right may be fighting a rear-guard battle it cannot win. But, aided by the growth in religiosity and by intermittent aid from the White House, the New Right has shown far more persistence and flexibility than any of its predecessors.⁴⁰

GIVEN THE PIONEERING CHARACTER of the historiography of conservative mass movements, it is not surprising that no persuasive synthesis of the subject has yet appeared.⁴¹ Until recently, many scholars on the right were, like the centrist liberals of the 1950s, either disdainful or suspicious of popular uprisings; as stern defenders of learned discourse, they did not warm to the millions of Americans who often articulated their hunger for a traditional moral order in fervent, revengeful ways. For their part, most advocates of the populist turn are, as relatively young scholars, too enmeshed in the monographic customs of the trade to attempt a broader interpretation.

Two recent volumes that do examine the modern American Right as a whole either add new details to the old extremist paradigm or focus on intellectual debates *about* popular politics rather than connecting ideas to the movements that embodied them. David Bennett's *Party of Fear* demonstrates the first tendency. A virtual textbook of counter-subversive attitudes and organizations, it begins with the anti-Catholic prejudices colonists brought with them from Elizabethan England and ends with George Bush, as vice-president, assuring Jerry Falwell, "America is in crying need of the moral vision you have brought to our political life." To his credit, Bennett specifies which ethnic groups, classes, and religious denominations prevailed in which organizations and explains that, depending on the region and period, a nativist bigot could be either a pro-labor reformer or a conservative farmer. But, apart from its arresting title, *The Party of Fear* fails to capture the drawing power of these movements, the ways in which they provided their adherents with a vision of an alternative America as well as a collective outlet

presidential candidate, Ed Clark, received more than one million votes, but the Libertarians have gradually lost membership and electoral appeal since then. For their doctrine, see Stephen L. Newman, *Liberalism at Wits' End: The Libertarian Revolt against the Modern State* (Ithaca, N.Y., 1984).

⁴⁰ On the issues at stake and the constituencies involved, see James Davison Hunter, *Culture Wars: The Struggle to Define America* (New York, 1991).

⁴¹ In recent years, some right-wing commentators themselves have adopted the term "movement conservative" to differentiate entrepreneurial organizers such as Richard Viguerie, the erstwhile wizard of direct mail propaganda, from officials in the federal government and Republican Party who spend their time trying to implement policy and win elections. This tendentious usage does point to a difference between idealists and pragmatists, but its assumption that the two groups have little in common is difficult to sustain.

for their anxiety and rage. On the other hand, Paul Gottfried and Thomas Fleming's concise survey of *The Conservative Movement* sympathetically recounts the major debates over philosophy and strategy in which various intellectual and political luminaries have engaged since the end of World War II. A valuable primer, it unfortunately makes no attempt to marry social and political history.⁴²

While much spadework into the grass-roots Right remains to be done, several larger questions can be identified that existing studies have ignored or minimized, matters that any sophisticated synthesis should consider. Three appear especially significant. First is the precise, evolving meaning of "traditional values." The new literature, whether discussing the Klan or the New Right, assumes too readily that a certain system of beliefs that valorizes moral self-discipline, monogamous nuclear families, and pious monotheism was constructed sometime during the Victorian age and survived, remarkably intact, into the 1980s. This is reminiscent of older debates about "the American character" that focused on content without analyzing the concept itself. Have there been no important substitutions in or reshufflings of the traditionalist package? Why have such ethics proved tenacious, renewing their appeal in periods like the 1920s, 1950s, and 1970s just when an amoral culture seemed to have triumphed? Much of the power of "traditional values" lies in the ability of their defenders to mobilize a variety of discontents under a consensual rubric that invokes both order and altruism, liberty and community. Why do some groups, such as devout Catholics during the Cold War and evangelicals more recently, welcome a politicization of their private and spiritual beliefs; while others, such as the fundamentalists of the 1920s, view the entire process as a corrupting one? We have thankfully abandoned the notion that the anti-modernist Right has appealed mainly to the rootless and the provincial. But we do not yet understand how it has deployed the most powerful weapon in its rhetorical arsenal.

Second, another of those weapons has been race, particularly white fears of the black presence. Surprisingly, none of the five books under review examines the ideology of racism, despite the self-evident linkage of the second KKK to its Reconstruction-era namesake and the New Right's heavy debt to George Wallace and other tribunes of the white backlash. Even Formisano, whose subject is a conflict over integration, takes up the issue of racism primarily to brand it a slur against blue-collar populists. "Elite whites," he contends, promulgated an "urban redneck myth" that held "the lower classes of American society . . . primarily responsible for racism, both overt and institutional."⁴³

While such stereotyping is undeniable, it does not negate the reality that right-wing movements, until the 1980s, usually defined themselves as refuges for

⁴² David H. Bennett, *The Party of Fear: From Nativist Movements to the New Right in American History* (Chapel Hill, 1988), quote on 401; Paul Gottfried and Thomas Fleming, *The Conservative Movement* (Boston, 1988). For a sound political narrative, see Michael W. Miles, *The Odyssey of the American Right* (New York, 1980). A longer discussion of Bennett's book (as well as a recent biography of Gerald L. K. Smith by Glen Jeansonne) can be found in my review, "Prophets on the Right," *The Nation*, February 21, 1989, from which some of the above paragraph is taken.

⁴³ Formisano, *Boston against Busing*, 233–35. Although Blee does include "racism" in her subtitle, the subject occupies only a two-page section that concludes, "Black people's lives were recognized only when they impinged on the lives of whites"; *Women of the Klan*, 157. In 1920, only 3 percent of Indiana's population was non-white.

ordinary people of a specific *race* and seldom made an effort to attract non-whites. To drain racial exclusivity, stated or implicit, from the appeal of the mass Right is to ignore the most critical division in the body politic, one the Left has long sought to eradicate or, in these multicultural days, to sanctify.⁴⁴ As Formisano does note, the ubiquitous term “forced busing” suggested the horror of interracial sex as much as the loss of parental control. One need not view race as the only factor behind the growth of groups like the KKK and ROAR to appreciate how the fear of black poverty, violence, and sexuality motivated both activists and followers. Only a small number of African-American conservatives have been able to separate the normative beliefs of the popular Right from its coded racial message.⁴⁵

Third, the new history of the Right needs an international perspective. Unlike their opponents on the Marxist left, American conservatives have generally rejected ties with similar movements outside the United States. “Americanism” has been a priceless property for the twentieth-century Right, and occasional admirers of European fascism like Charles Coughlin earned only scorn and then oblivion for their stance. But conservative populism has hardly been limited to the United States—as Margaret Thatcher’s Tories, the Front Nationale of Jean-Marie Le Pen, and, albeit in a different context, the Catholic politicians of post-Communist Eastern Europe have recently demonstrated. A self-defensive pride in one’s (presumably homogeneous) national culture, strong support for “the family” and church, and a mistrust of cosmopolitan, left-wing intellectuals and social planners are common to all these groups. While the resources available to each and the purposes being served obviously differ tremendously, the transnational political success of anti-modernist, fiercely nationalistic leaders and parties is striking. Similarly, in the 1920s, avowedly racist, pro-family movements gained millions of adherents on both sides of the Atlantic Ocean. But the KKK was too wedded to a localist, anti-state ideology to find the revolutionary authoritarianism of German Naziism or Italian fascism attractive.⁴⁶

Even to suggest such an odious parallel is to raise the unavoidable issue of the historian’s moral responsibility. It is true that contributors to *The New American*

⁴⁴ For criticism of the tendency of “new” labor historians to downplay race and racism, see my article, “A People Not a Class: Rethinking the Political Language of the Modern United States Labor Movement,” *Reshaping the US Left: Popular Struggles in the 1980s*, Mike Davis and Michael Sprinker, eds. (London, 1988), 257–86; and David Roediger, “‘Labor in White Skin’: Race and Working-class History,” *ibid.*, 287–308.

⁴⁵ Formisano, *Boston against Busing*, 119. For an excellent study of how the Republican Right has used white anxieties from the 1960s to the 1990s, see Edsall and Edsall, *Chain Reaction*. A neglected theoretical work that explores “the irreducible political aspect of racial dynamics” is Michael Omi and Howard Winant, *Racial Formation in the United States: From the 1960s to the 1980s* (New York, 1986), quote on 4.

⁴⁶ On the other hand, women active in the Klan and the German National Socialist Party both attempted to construct what Claudia Koonz (referring to the Nazis) calls “a social space where domestic tranquillity and traditional values reigned.” Both enjoyed a good deal of autonomy within a movement led by anti-feminist men. Koonz, *Mothers in the Fatherland: Women, the Family, and Nazi Politics* (New York, 1987), 107 and *passim*; Blee, *Women of the Klan*. Alan Brinkley cogently argued that the world-views of Huey Long and Charles Coughlin (despite his kind words for Mussolini, Franco, and Hitler in the late 1930s) had little in common with European fascism. Brinkley, *Voices of Protest*, 269–83. On the significance of transnational ideologies generally, see Ian Tyrrell, “American Exceptionalism in an Age of International History,” *AHR*, 96 (October 1991): 1050–53.

Right missed the commonplace complexity of the people and causes they rushed to belittle. But, as “public intellectuals,” they took seriously their obligation to warn readers about the potential for despicable words and conduct that existed within mass movements, on the left as well as the right. The new scholars of grass-roots conservatism who are more or less securely lodged within academia might be more conscious of the point at which their empathy for a group like the Klan or ROAR shades into retrospective esteem—and whether they want their work to serve that purpose. There is no need to adopt a stance of the relativist or political agnostic when writing about controversial movements. Contemporary historians seldom believe anymore that they can or should try to capture “the truth.” But Gordon Wright’s credo from the mid-1970s is still worth hearing: “for some of us at least, our search for truth ought to be quite consciously suffused by a commitment to some deeply held humane values. The effort to keep these two goals in balance may be precarious; but if we manage it, perhaps we will be on the way to re-establishing the role of history as one—and not the least—of what we might fairly call the moral arts.”⁴⁷

⁴⁷ Gordon Wright, “Presidential Address: History as a Moral Science,” *AHR*, 81 (February 1976): 11. The term “public intellectual” is borrowed from Russell Jacoby’s splendidly provocative *The Last Intellectuals: American Culture in the Age of Academe* (New York, 1987).

Reviews of Books

GENERAL

KIRKPATRICK SALE. *The Conquest of Paradise: Christopher Columbus and the Columbian Legacy*. New York: Alfred A. Knopf. 1990. Pp. 453. \$24.95.

In Kirkpatrick Sale's eloquent and passionate treatment, Columbus and the Columbian legacy become a paradigm for the ruthless expansion of Western Europeans overseas. Western and Mediterranean Europe developed an energy-hungry civilization different from cultures in Asia, Africa, and America. The conquerors, according to Sale, consumed too much meat, were reluctant to adapt to more productive local crops, had a callous attitude toward women, and, in the first centuries of contact, possessed a mind closed to any understanding or appreciation of native interpretations or conceptions. In a blundering, heedless course, Europeans settled large parts of the globe without regard to the rights of natives, devastated huge areas, violently disturbed or replaced native biota, and now threaten to leave a gutted, overpopulated, and seriously impoverished planet.

In contrast, native Americans before Columbus lived essentially in harmony with their environment. They extracted sustenance from the biota while maintaining it; they made shrewd use of native plants and held down natural population increase. In general, women enjoyed much higher status than in Western Europe and, in many tribes, inheritance moved through them. Warfare was infrequent. Much of Sale's description pertains to what is now Anglo-America, for Mesoamerica and the Andes had developed state systems with high degrees of social stratification that meant wars, expansion, and (in Mesoamerica) destructive exploitation.

According to Sale, Columbus's voyages initiated the period when Europe began its overseas expansion and eventual domination of the planet. A large part of the book examines the myth-shrouded enigma of the Genoese sailor who became Admiral of the Ocean Sea. Sale has carefully studied the writings of Columbus's contemporaries and the admiral's later writings, which have only recently become accessible, and through rigorous analysis declares baseless such

myths as the admiral's lifelong conviction that he had reached Asia. Sale points to personality quirks, questions Columbus's religious zeal, and denounces his brutal exploitation of the Taino. Following Columbus's voyages, Spain developed a vast American empire and a consonant administrative structure. With huge revenues from American mines, the Spanish Hapsburgs could attempt to reorder Europe, exhausting their newly won resources in a century-and-a-half of incessant wars.

A particularly fine part of the book explores changes in Columbus's reputation, as found in histories, biographies, poems, and literary prose, in Spain and other countries. Only in 1892 did the anniversary of his achievement excite great attention, as it will in 1992, but now with much challenge from other points of view.

One example Sale cites of how Europeans adopted the Columbian legacy is in the British colonies, particularly in Virginia and New England, where he finds the same callousness toward the natives and the same reluctance to adapt. This behavior was distinguished from Spain's only by the lack of any protesting voice such as Bartolomé de las Casas and by the fiercer attitudes that Protestantism engendered.

Sale's examination of Columbus has a hard-minded quality that cuts through much obfuscation and legend. He emerges as a more human and complex figure, but this treatment also anachronistically applies views that come from the native American and women's rights movements, environmentalism, vegetarianism, animal rights, and so on, so that European civilization becomes the Great Satan of time and the planet. I must point out that the destruction of the environment by humans can easily be found in the Middle East, Africa, and Asia, where much desertification has occurred; nor have the inhabitants of those areas been distinguished by concern for the rights of other peoples, women's rights, or animal rights. The most that can be said is that they did not invariably have the dynamic of Western Europe. As for the native Americans, Sale's picture of paradise is overdrawn. Mesoamerica apparently went through cycles of over-exploitation of resources and population col-

lapse. In short, Sale has written a work of considerable virtues as well as defects.

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IAN HACKING. *The Taming of Chance*. (Ideas in Context, number 17.) New York: Cambridge University Press. 1990. Pp. xii, 264. Cloth \$44.50, paper \$14.95.

Ian Hacking is one of a small group of innovative and talented philosophers investigating conceptual problems in science and mathematics from a wide-ranging historical perspective. This book is a sequel to his *Emergence of Probability* (1975), which documented the way in which probabilistic reasoning developed in the seventeenth century. The new volume is a more complicated tale about the longer transformation that, Hacking argues, began at the end of the eighteenth century and was not complete until the early twentieth century. In this period metaphysical necessitarianism and universal causality were eroded and gave way to indeterminism.

Hacking acknowledges that his emphasis is not history but epistemology, and his thrust—or perhaps lack of concern for narrative—makes his brief book a strain to understand. He has something in common with his favorite philosopher, Charles Peirce, whose doctrine of universal chance Hacking discusses at the end of the volume. Peirce's notoriously difficult writings have also baffled interpreters.

Hacking has mastered several vast and complicated literatures in three languages. He has covered not only the mathematical studies pertaining to his topic in nineteenth-century Europe but has also examined the mass of information generated by various government bureaus and amateur data collectors. The book unearths fascinating material on the study of suicide (of which Emile Durkheim's great work of 1897 is only the most famous), on attempts to conceptualize a perfect jury system, and on the statistics of anti-Semitism.

As I grasp Hacking's argument, during the nineteenth century an avalanche of numbers was produced. The growth of manufacture and trade, the technology of war and empire, and mass production and bureaucracy drove experts to collect and systematize numerical material about all aspects of human life. As the century wore on, these data were sophisticatedly disaggregated. Although Hacking does not slight medical statistics, he does, it seems to me, emphasize less than he might the stunning example of the role that the hospital played in acclimating the public to chance and probability. The agglutination of large numbers of sick people enabled arithmetically minded doctors to codify much useful evidence about the types and course of illness. Ultimately this information was helpful in controlling and understanding disease.

A persistent theme that runs through this book is the distinction between Anglo-French and German data collection. For the "West," the regularities uncovered were corroboration of a lawlike universe because physical laws were considered to be nothing more than regularities. For the Germans, these regularities could never be the basis of law, which concerned real causes acting on individual events and necessarily producing their effects. For the English, therefore, statistical laws evidenced fatalism, although deep analyses of causality were eschewed. Where such analyses were licit (in Germany), statistics corroborated the belief in freedom; statistics suggested that human behavior was not truly lawful.

Overall, by the twentieth century philosophers and scientists had committed themselves to a probabilistic universe in which chance was real. Statistical laws reflected the genuine nature of the universe. Yet, although some things were inherently unpredictable, it was also rationally justifiable to believe in the reality of statistical regularity.

Hacking sees the growth of "social" numbers as constantly interacting with the evolution of indeterminism in science and with cogitation about the nature of statistic inference in philosophy. The result in the twentieth century is a culture that believes both in chance and in "the percentages."

My summary hardly does justice to the richness of Hacking's ideas or his many asides, like the striking comparison of Peirce and Friedrich Nietzsche. As Hacking suggests, he is indebted to Michel Foucault's attempts to write a broad social history of ideas that uncovers the roots of basic cultural preconceptions in a plethora of institutions and that shows how these preconceptions change over time. Nonetheless, Hacking's meticulous scholarship, his comprehension of various areas of learning, and his commitment to linear exposition seem to me to exceed Foucault's. At the same time, Hacking is not an anti-Foucauldian model of clarity. Historians like me will be driven to distraction by the penchant for irrelevant anecdotes, which the author mistakenly believes humanize his treatment of many of his subjects. This lack of good sense about how to write a story about the past is more seriously mirrored in the random zigs and zags in Hacking's account that make his philosophical points hard to follow. The struggle is nonetheless worthwhile.

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HERBERT MEHRTENS. *Moderne—Sprache—Mathematik: Eine Geschichte des Streits um die Grundlagen der Disziplin und des Subjekts formaler Systeme*. Frankfurt a.M.: Suhrkamp. 1990. Pp. 640.

The paradise of mathematicians, imagined by Carl Friedrich Gauss in a letter of 1802, is a place in which *Geist* is no longer confined by space nor chained to a

ponderous, suffering body. Herbert Mehrtens cites this marvelously revealing and apt line as a prelude to mathematical modernism. The comment provides also a prelude to the historiography of mathematics, which has been dominated by mathematicians and largely oblivious to the earthy circumstances of body, place, time, and human passions and interests. Professional historians would be unlikely to pay attention to such history even if it were not rendered inaccessible to most by its technicality. Yet the cultural history of mathematics is a subject of real historical importance, as this book demonstrates.

At first it might not seem so, for, as Gauss's remarks suggest, modernism in mathematics has meant a retreat from the world of flesh and blood, and even of space and time. Characteristic is David Hilbert's view of non-Euclidean geometries, which Mehrtens explains with admirable clarity. Others, especially of an older generation, were deeply troubled by the pall of uncertainty that these new geometries cast over mathematical knowledge. If there are several rival geometries, how could one know which was the true one? Hilbert, the most important and influential mathematician of this century, was unfazed. All candidate geometries are true—mathematically. It is of little import whether they are also true physically. "The new language of mathematics does not need to be made certain in relation to an exterior reality, because it makes itself certain through its own work" (p. 68). Whatever is shown by research to be logically consistent is therefore true; mathematical truth means precisely this. Mathematics is a *Symbolsprache* that refers to nothing outside itself and that can remain serenely indifferent to mere matters of science. With a magisterial *Es sei x . . .* ("Let x be . . .") it creates its own world, more logical and therefore more beautiful than anything recorded in the first chapter of Genesis.

Mathematics is a fine exemplar of contemporary discourse because it is closed and inward looking. That inwardness is supported by high disciplinary barriers, also characteristic of our time. Mehrtens, in good modernist fashion, identifies mathematics as a language, one that we cannot freely speak because instead it speaks us. He suggests that this depiction more accurately reflects the highly structured and rule-bound discourse of mathematics than any natural language. Literary scholars now talk routinely of the "death of the subject," but surely this should be identified first of all with mathematics and the sciences rather than with novels and poetry. Mehrtens thinks so, although with an important qualification: the exclusion of personality prevails mainly in mathematical *Sprache* (language). It is greatly attenuated in the less-formalized, less-public domain of *Sprechen* (speech). And one might well believe this had Mehrtens not left the notion of *Sprechen* itself so vague and disembodied.

It would not be fair, however, to imply that Mehrtens has brought into his history the abstractionism of

the people he studies. Their work, we learn here, had consequences that were material and technological as well as cultural. For example, the isolationist mathematics of Hilbert, which tended to idolize the reasoning machine, was in crucial ways the source of actual reasoning machines, modern computers. Fittingly, computers continue to symbolize this ideal of the disembodied mind. More generally, mathematical modernism was Janus-like in its stance toward human interests. The so-called *Gegenmoderne*, who opposed Hilbert and his allies, shared with modernists the crucial belief that mathematics is not first of all descriptive of an outside world. Antimodernists identified mathematics with human intuitions and representations (*Anschauungen*) and opposed what some called "Jewish" formalism. This connected with a language of proto-Nazism, which Mehrtens explores. It also made mathematics first of all a servant of human interests and needs and provided a guiding philosophy for the promotion of applied mathematics. Felix Klein, Hilbert's antimodern counterpart at Göttingen, was especially influential in this regard.

This book is sprawling, unwieldy, and uneven. Some arguments are repeated too often; others are left isolated and dangling. I wish Mehrtens had said more, for example, about the cultural significance of mathematical rigor and impersonality in the wider domain of public life. But if the book is somewhat unstructured, even inchoate, it is also highly ambitious and original. While it is mainly about Germany, its implications are far-reaching. And—no small achievement—Mehrtens has succeeded in writing a sophisticated study about mathematics without demanding any mathematical knowledge of his readers. This is history of mathematics that matters for historians and humanists, and indeed for anyone interested in the (post)modern intellectual scene.

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MARCEL C. LAFOLLETTE. *Making Science Our Own: Public Images of Science, 1910–1955*. Chicago: University of Chicago Press. 1990. Pp. ix, 260. Cloth \$45.00, paper \$17.95.

I write this review while the nation is awash in the media's fifth week of romanticized, high-tech war symbols. Smart bombs and precision lasers construct an image of Middle Eastern conflict in sanitized Walt Disney-style. Such images depend on, and simultaneously reinforce, a longstanding mythology of science and technology. Their prevalence underscores the importance of studies such as this one. Marcel C. LaFollette has written a popular book, read best when read quickly. Such a tack lets the reader both benefit from her helpful general themes about the changing content of science articles and take advantage of the

broad sweep of her evidentiary base: 687 nonfiction science articles sampled from eleven general audience, high-circulation magazines over nearly half a century.

A quick read yields considerable take-home value. It puts provocative textual flesh on LaFollette's thematic descriptions of the relationship between science, media, and public opinion. The media, she finds, tended to conflate scientists (intelligent, persistent, modest, forward looking, and male) with science. Women scientists, for their part, simultaneously appear as atypical scientists and atypical women. The male scientist's family offers support while the female scientist's family depends on her capacity for double duty at home and at the laboratory. Personifying and individualizing science tended to romanticize scientific practice for much of the period. Only after 1940 do science writers begin to convey images that emphasize method over equipment (especially chemical apparatus) and teams over individuals. In the book's best section, chapters 8–11, she gathers such themes into her primary hypothesis and connects them with her contemporary agenda. LaFollette writes from her concern over the recent "philosophical and political tension now existing between science and the public . . . that is expressed in increased government and social regulation." For this she blames "muddy descriptions of how science really worked" together with excessive hype ("when journalists hailed each scientific discovery as a 'whisper of salvation' and cited each success as proof that more good things would come"). Betrayed expectations gradually fostered a public backlash (p. 183).

LaFollette's themes seem almost commonplace when looked at apart from the cited texts. Nevertheless, the book teaches not so much by challenging understandings of how science and media operate as by deepening the sense of how those operations taste. Romanticized and demonized, science has acquired a profound visceral presence in society. LaFollette offers the reader the chance to parse science's rhetorical grammar without losing touch with its emotional power.

Unfortunately, the book's argumentation and rhetoric suffer from a closer reading. Infelicitous expressions blur some of LaFollette's arguments. Imprecise source notes and tables do not always establish the precise argument in question (for example, chapter 1, n. 49, or the citation of figures 3.5 and 3.6 on page 53). Occasionally, as in her discussion of Charles Proteus Steinmetz, LaFollette uses popular magazine articles from her data base as the primary source for her own historical analysis (p. 55). In short, this very helpful and informative book could have been much improved with a rigorous critique of rhetorical terms and scholarly apparatus.

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LUCETTE VALENSI and NATHAN WACHTEL. *Jewish Memories*. Translated by BARBARA HARSHAV. Berkeley and Los Angeles: University of California Press. 1991. Pp. v, 351. \$30.00.

This English translation of Lucette Valensi and Nathan Wachtel's *Mémoires Juives* (1986) is a beautifully wrought book that seeks to re-create the lost worlds of Jews who made their way to France after World War I. Based on the oral testimonies of thirty-six principal informants from the Levant, North Africa, and Eastern Europe, this book is not about the immigration experience per se. Rather, it evokes life in communities that only decades ago constituted vibrant centers of Jewish existence. It also offers a fascinating glimpse into the mechanisms by which westernization and immigration radically transformed traditional Jewish society.

The authors have made a deliberate effort not to interject their own views too directly into the text, although whether they have succeeded is a key question to which we will return. Thus, the stated aim of the book is to allow the voices of the informants to speak for themselves. Aside from editing the interviews and rearranging them thematically, scholarly commentary is kept to a minimum and background information is relegated to footnotes. The primary emphasis of the book is on social and cultural relations; political activities, although alluded to, are decidedly of secondary importance. While the authors have granted equal time to the Sephardi and Ashkenazi experiences, English-speaking readers will most likely find the descriptions of Sephardi culture most intriguing, if only because that culture is far less familiar.

The first part of the book, "The World of Yesterday," vividly re-creates a sense of life as it existed in the insular Jewish communities of North Africa, the Levant, and Eastern Europe. This is the world of the ghetto, where socially and culturally, if not economically, Jews lived in an exclusively Jewish milieu. They spoke their own languages, ate their own foods, practiced endogamy, and observed the precepts of their own religious tradition. Although some informants found this insularity stifling, the vast majority recollect this past with affection. As the authors comment: "Far from complaining about being confined, they recall a rich world where social relations were intense and incessant" (p. 23).

The second and third parts of the book, "Passengers in Transit" and "The Others," describe the impact of westernization and secularization prior to emigration as well as the mutual disdain and exclusiveness that separated Jews from their non-Jewish neighbors. The Jews of North Africa and especially of the Levant were an extremely cosmopolitan element. As one respondent from Cairo recalls, the terms "Jews" and "Europeans" were virtually synonymous, and middle and upper-middle-class Jews were often fluent in three or four languages. Remarkably, how-

ever, Arabic was rarely learned. Social interaction between Muslims and Jews is likewise recollected as virtually nonexistent, despite the fact that the two groups lived in close proximity. One woman from a well-to-do Istanbul family recalls that while growing up in the 1920s she never once spoke to her Muslim neighbors. That Jews in Muslim lands generally attended foreign schools and purchased legal protection from the various Western powers at hand exacerbated this social distance. In Eastern Europe, too, although Polish culture exercised a greater attraction, it by no means constituted the sole medium through which Jews experienced modernization. Rather, a system of innovative Hebrew and Yiddish schools created between the wars permitted Jews to modernize without assimilating to Polish culture. Moreover, as several informants recall, the persistent strength of Polish anti-Semitism, even among the ranks of the socialist party, tended to encourage a separate Jewish political movement.

The final section of the book, "Exile and Mourning," discusses the impact of Vichy anti-Semitism and the Holocaust and includes some bitter reflections on life in France. France is depicted as inhospitable and alienating, the polar opposite of the Jewish society left behind. The French, the authors comment, "look at you without seeing you and kill you by a kind of slow death. Exchanging neither words nor looks, they don't share wine or bread either" (p. 273).

While statements of this kind clearly echo those of the informants themselves, they also suggest that the authors have allowed their personal biases to shape the presentation of the material. Indeed, the entire book is suffused with a nostalgia for the lost world of the ghetto. Time and again, the authors refer to this world as a "harmoniously integrated" totality where social relations were intimate and "intense." The negative aspects of this existence—the extreme poverty and the oppression of women—are greatly minimized. Commenting on the reminiscence of one Polish informant that "in spite of poverty, we were happy," the authors raise the key question of whether this nostalgia might simply be "retrospective illusion" (p. 89). But instead of answering this question critically, they indulge in romanticizing the past themselves. The very structure of the book reflects this nostalgia: it moves from the integrated and harmonious "World of Yesterday" to the world of death and alienation depicted in "Exile and Mourning." Moreover, what is being mourned here is not simply the toll exacted by the Holocaust and Arab nationalism; it is also the price paid for modernity itself. As to any gains, there seem to be none. One can only wonder whether a book on Jewish immigration to the United States or to Israel would have ended on such a deeply pessimistic note. This criticism notwithstanding, this book, expertly translated by Barbara Harshav, provides a rich repository of data on local Jewish cus-

toms, traditions, and family life that will be of immense value to anthropologists and historians alike.

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NECHAMA TEC. *In the Lion's Den: The Life of Oswald Rufeisen*. New York: Oxford University Press. 1990. Pp. x, 279. \$21.95.

Were this book not so heavily documented, one would be inclined to dismiss it as the musings of an incredibly creative mind. Were it a novel, one might be inclined to suggest that Nechama Tec has strayed into the realm of the improbable.

During the Holocaust, Oswald Rufeisen, a Polish Jew, passed as a Christian. He worked for the division of German police responsible for rounding up Polish Jews for transportation to death camps. Fluent in German and Polish, Rufeisen served as a translator assigned to question people to determine whether they were Jews. A favorite of the German police commander, he was treated as a surrogate son and granted many privileges. Rufeisen used these to warn Jews about roundups. He helped Jews in the Mir ghetto obtain arms, which they used to stage a revolt. He protected an entire village from destruction.

This alone would make Rufeisen's story remarkable. But this is only the beginning of a complex journey. When the Germans discovered Rufeisen's identity he escaped to a Polish convent. Overwhelmed by the faith of the sisters, he converted to Catholicism.

After the war he became a Catholic priest. Unlike other Jewish converts to Christianity, he did not reject his Jewish identity but professed to be a Jew by nationality and a Christian by faith. To further complicate matters he also openly proclaimed his Zionist beliefs—all this in Communist Poland.

Anxious to fulfill his Zionist yearnings, he petitioned for permission to go to Israel. Rufeisen informed both Polish and Israeli authorities that it was his Jewish nationality, Zionist beliefs, and Christian commitments that compelled him to move. After numerous rejections by both governments he finally won permission to emigrate. Israeli authorities reluctantly granted him a one-year visa, although the ambassador to Poland acknowledged that no one would ask him to leave when the year ended.

But Rufeisen, now known as Father Daniel, was not satisfied. He wanted to enter Israel under the Law of Return, which stipulated that Jews immediately be granted Israeli citizenship on request. The only requirement was that they be Jews. The law, passed in the shadow of the Holocaust when Jews found most countries closed to them, left it ambiguous as to who was a Jew. Father Daniel challenged the court to recognize him as a Jew. After a long and drawn out deliberation his request was denied. National and international interest in the case was immense.

Had Rufeisen not compiled such an outstanding record during the Holocaust and had he not been directly responsible for rescuing so many Jews his suit would have been dismissed as frivolous and even insulting. Jews in general and Israel in particular have little sympathy for Jews who convert, particularly when they actively embrace a faith that many Jews believe is, at the least, indirectly responsible for the crimes of the Holocaust.

Tec conveys not just the adventure but also the importance of this story. In this book, Tec, who has already made major contributions to our understanding of the behavior of Christians who rescued Jews during the war, further illuminates the puzzle of the existence of goodness in the midst of evil and of the complex relationship between Christianity and Judaism that was manifested during these trying times.

DEBORAH E. LIPSTADT
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AVIEL ROSHWALD. *Estranged Bedfellows: Britain and France in the Middle East during the Second World War*. (Studies in Middle Eastern History.) New York: Oxford University Press. 1990. Pp. xii, 315.

This is a well-written scholarly study of the relationship between Britain and France within the context of the Middle East during World War II. Until recently most of what we knew about the subject was contained in the memoirs of Charles de Gaulle, Winston Churchill, E. L. Spears, and other lesser figures. In 1987, Philip S. Khoury published his detailed study of Arab nationalism from 1920 to 1945 and A. B. Gaunson published *The Anglo-French Clash in Lebanon and Syria, 1940–45*. Now we have Aviel Roshwald's book, which covers much the same ground as Gaunson's study but with the added benefit of access to recently declassified French archival material. The result is a welcome addition to an expanding literature on the subject.

Roshwald's study begins with an examination of the ambivalent British policy toward Vichy-controlled Syria and Lebanon in 1940–41. Seeking to avoid a direct military confrontation, the British supported the Free French in their attempts to undermine Vichy's control of the region. After Vichy offered Syrian air bases to the Germans during the anti-British revolt in Iraq, British troops, with Free French support, seized the Levant. This created the ambiguous situation that forms the backdrop for Roshwald's book; de Gaulle's movement exercised juridical authority in Syria and Lebanon, but effective military power resided with the British.

This anomalous situation threw into stark relief the fundamental tensions that had characterized Franco-British relations in the Middle East since the late nineteenth century. France's defeat in Europe in 1940 magnified the importance of its empire as a source of national self-esteem and grandeur—a posi-

tion frequently asserted in grating fashion by de Gaulle. The British, however, were increasingly tempted to meddle in local affairs once their forces were in effective control. French officials viewed British interference as an effort to replace them in the Levant. The overlapping authorities in the region, therefore, led the British and the Free French to pursue vindictive policies toward each other that belied their mutual commitment to the wartime alliance.

Roshwald focuses considerable attention on the personal diplomacy of Major-General E. L. Spears, the British chief of mission in Syria and Lebanon. Spears's close relationship with Churchill made it possible for him to chart a course virtually independent of the Foreign Office. Although the British Foreign Office officially supported the continuation of French influence in the Levant, Spears's antipathy to de Gaulle and his political ambition played a decisive role in the formation of his own conception of British interests in the region.

The author is careful, however, not to overinflate the importance of Spears's personal role. He concludes correctly that Spears's overtly hostile conduct made the French perceive the Syrian and Lebanese declarations of independence as the direct result of a concerted and preconceived plan to oust them. More than anything else, the French collapse is attributable to its own underestimation of the forces of nationalism in the area, a fact that underscores the need for a definitive work on Syrian and Lebanese political developments during the war.

Without Spears's constant incitement, the Syrians and Lebanese would have been unlikely to challenge French authority so boldly in the midst of war. But France's collapse in the Middle East was a foregone conclusion in any case, although it probably would have occurred more gradually, as did Great Britain's. Given the grand panorama of World War II, the struggle over Syria and Lebanon was a marginal sideshow. The issues, however, stood in the way of cooperation between the British and French over the broad community of interests they shared in the European sphere.

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RANDALL BENNETT WOODS. *A Changing of the Guard: Anglo-American Relations, 1941–1946*. Chapel Hill: University of North Carolina Press. 1990. Pp. xi, 473. \$42.50.

Randall Bennett Woods's book is the best and most thorough account of the wartime Anglo-American alliance since the publication in 1956 of Richard N. Gardner's classic study, *Sterling-Dollar Diplomacy*. Like Gardner, Woods follows American policy makers as they tried to break down the British system of imperial preference and replace it with a global system of

multilateral trade and convertible currencies. He surveys the various initiatives, including the Lend-Lease agreement of 1942, the Bretton Woods agreements of 1944, and the American loan to Great Britain in 1946, through which American officials pushed the British toward multilateralism. In the process, he does a superb job of delineating the influence on Anglo-American diplomacy of ideological imperatives, factional politics, key personalities, and bureaucratic rivalries. In many ways this is the best and most original feature of a book that is buttressed throughout by meticulous research in British and American sources and that takes a sharply critical view of U.S. policy.

For Woods, the American search for multilateralism actually masked what soon became a drive toward global economic hegemony—a drive that would retard postwar recovery in Europe and make it difficult for policy makers in Washington to achieve their strategic and geopolitical objectives. By stressing these and other shortcomings, Woods echoes the earlier criticism of New Left scholars, who also viewed the American brand of multilateralism as essentially a variant of economic nationalism and imperialism. This is the case even though Woods's interpretation is basically more conservative than that of his New Left predecessors. His biggest complaint is that American policy makers often tolerated Soviet expansion while they attacked the British system of imperial preference. Moreover, they pursued policies that weakened the British at a time when strong allies were needed to contain the Soviet Union.

Serious books should be seriously engaged, especially when they are likely to become a standard account of an important subject. Such is the case with Woods's study, which raises several concerns as important as the book itself. Woods underestimates the desire of key British policy makers to move toward the multilateral world that American leaders had in mind as well as their ability to gear the pace of this transformation to their own, rather than American, needs. After all, the pound sterling did not become fully convertible until the late 1950s, more than fifteen years after the Lend-Lease agreement. Similarly, Woods exaggerates the pressure that American leaders put on the British government and slights their willingness to relax this pressure when the results seemed harmful to long-term British and American interests. It is important to remember in this connection that the Anglo-American loan agreement of 1946 followed the Lend-Lease agreement of 1942, just as the Marshall Plan would follow the Anglo-American loan. In other words, whenever the British began to buckle and break under the weight of economic hardships the Americans would relax the pressure for multilateralism and arrange additional aid to their beleaguered ally.

Because Woods ends his story with the Anglo-American loan it might seem unfair to mention subsequent American initiatives, such as the Marshall

Plan. But doing so points up what strikes me as the most significant flaw in this significant book. Woods writes about the period 1941–46 as if these years were cut off from all that preceded or followed them. Missing is the broader perspective that would come from viewing Anglo-American diplomacy during the war years as part of a longer historical process by which the United States moved from the isolationism and nationalism of an earlier day to the liberal internationalism of the postwar period. For example, by stressing the concessions that policy makers made to their critics in Congress, Woods leaves the mistaken impression that U.S. diplomacy during the New Deal and Fair Deal was primarily a triumph for conservative nationalists and economic isolationists. In his account, that is, the Reciprocal Trade Agreements Act, the Bretton Woods agreements, and the Anglo-American loan agreement all appear as serious setbacks rather than as signposts along the bumpy road to multilateralism.

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RITCHIE OVENDALE. *Britain, the United States, and the End of the Palestine Mandate, 1942–1948*. (Royal Historical Society Studies in History, number 57.) Rochester, N.Y.: Boydell, for the Royal Historical Society, London. 1989. Pp. 332. \$71.00.

This is an important historical study that helps us to understand the highly complex peace process that the Bush administration is attempting to effect in the Middle East. It is not open to discussion that the United States today is the single most influential external power in determining the outcome of the hitherto conflictory relations between Israel and the Palestinians, and, of course, the relations between Israel and the Arab states. Scholars of the Middle East as well as other analysts recognize the secondary role that Great Britain continues to play in the area; that role is essentially supportive of America's policy.

Ritchie Ovendale's study is tightly focused: he details the diplomatic process that brought about the inevitable termination of the British Mandate in Palestine (May 1948), which also signaled the gradual ascent of the United States as a power in the region. Although the decline of Britain's power came about with increasing violence in Palestine itself, Ovendale's careful study is more concerned with Britain's international political efforts to pursue a political solution to the question of Palestine that was acceptable to both the Palestinians and European Zionists. Such efforts clearly had to take into serious account the evident growing interests, influence, and power of the United States. Much of Ovendale's study is concerned with the interaction between Britain and the United States, either bilaterally (through their embassies) or through their efforts and policies in the

United Nations, where the international struggle over Palestine was being waged.

The mandate, including the commitment entailed by the Balfour Declaration of 1917, was imposed on Palestine in 1922. Between that time and 1942, the United States was only minimally involved in Palestine. By 1942 it had become clear that the United States was emerging into its global status, and of course the Zionists had, through the Biltmore Program, signaled their shift of policy to wage their battle for support in the United States. Ovendale correctly identifies this crucial period as one marking not only the decline of Britain's power in Palestine but also the ascent of the United States to the position of prominence that it continues to occupy.

Through exhaustive combing of British and American archives, diplomatic exchanges, unpublished private papers, original documents, and through a clear understanding of the actual unfolding of events derived from the standard secondary works, Ovendale is able to identify the most important political players and their positions: Ernest Bevin, Harold Beeley, George Marshall, Robert Lovett, James Forrestal, Loy W. Henderson, Clark Clifford, David Niles, and the diplomatic missions in the Arab states. But the decisive player clearly was President Harry S. Truman, who had to assimilate and synthesize the conflicting domestic and foreign pressures.

The challenge to President Truman was a complex one: how to get elected (domestic politics) while simultaneously conserving and strengthening the Anglo-American relationship and preempting a possible Soviet advantage in Palestine. Truman's crucial decisions—support for the admission of 100,000 European Jewish displaced persons to Palestine, backing of the UN resolution partitioning Palestine, and recognition of Israel—reflected the greater influence of those who thought of Palestine in terms of domestic politics. Their opponents believed that Secretary of State Marshall should and could remove Palestine from domestic politics and give it "its rightful place as a dangerous and difficult international problem" (p. 295). They believed that Secretary Marshall could persuade Congress, the media, and other supporters of Zionism to forgo the political use of Palestine in domestic politics. They were wrong then, as their successors are today. The principal policy issues and findings of this important study have been analyzed differently in previous works that addressed either Britain's or America's policy in the Middle East. Ovendale's is more detailed, extensively documented, and more exclusively focused on the interaction between the two powers than previous works. Thus, it is a welcome addition to Roger Louis's *The British Empire in the Middle East 1945–1961* (1984) and Dan Tschirji's *The Politics of Indecision: Origin and Implications of American Involvement with the Palestine Problem* (1983).

IBRAHIM ABU-LUGHOD
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JAMES BARROS. *Trygve Lie and the Cold War: The UN Secretary-General Pursues Peace, 1946–1953*. DeKalb: Northern Illinois University Press. 1989. Pp. x, 445. \$37.00.

James Barros previously has published books on the two secretaries general of the League of Nations, Sir Eric Drummond (1919–33) and Joseph Avenol (1933–40). The present volume on Trygve Lie, first secretary general of the United Nations, continues the high quality of its predecessors. It is thoroughly researched, well written, and on the whole even-handed. In addition to being about Lie, it is, as the title indicates, an analysis of the origins of the Cold War as waged within the United Nations.

Barros's account of how Lie was elected secretary general is intriguing, particularly with regard to the role of Alger Hiss. Barros suggests that Hiss—whose own candidacy for the position had been proposed informally by a Soviet official—may have been responsible for the appearance of the Norwegian's name on a "personal list" that American representative Edward R. Stettinius, Jr., put forward without consulting Washington. Lie, Barros shows, had been Moscow's choice all along. Not that Lie was anyone's agent, it should be added, but the Soviets considered him to be the person most considerate of their interests whom the Western powers would accept.

Lie did tilt toward the Soviet Union, at least during his first years in office. To what degree his attitudes grew out of his socialist background, his concern for Norway's proximity to the Soviet Union, or from his dedication to making the United Nations work by building bridges between the two blocs can only be guessed. In any event, Barros writes, Lie developed "two sets of hurdles, in which the Russian one was always several pegs lower, despite Lie's denials that this was so" (p. 36).

Determined that the UN would not go the way of the League, Lie was an activist secretary general from the start. He used the full powers of his office and more. He took initiatives on issues publicly and privately even when no member states invited him to do so. He succeeded in creating a high profile for the UN (and for himself), but at a cost. To the extent that he became an advocate, he diminished his value as an honest broker. Like a referee at a sports event, he sometimes ended up being criticized by both sides.

North Korea's invasion of South Korea in June 1950 traumatized Lie. An operation of such size had to have been carefully planned and had to have had at least the concurrence of the Soviet Union. To Lie, the assault was reminiscent of Nazi Germany's invasion of Norway in 1940, and he believed that failure to respond militarily would spell the UN's doom. His active role in orchestrating the organization's response earned him the Soviets' undying hostility. Over the next few years they simply refused to acknowledge that he or his office existed. Lie's use-

fulness was further compromised by his increasing willingness to cooperate with U.S. "loyalty" investigations of American nationals employed by the UN secretariat. He announced his resignation in November 1952, but stayed on until a successor was chosen in April 1953. His departure went unlamented.

Lie frequently told people his job was "impossible," and so it was. The UN never became a truly international organization within which disputes could be settled by consensus (except on those issues in which none of the major powers had a stake). Instead, it became yet another arena of the Cold War, as the contestants incessantly jockeyed for political and propagandistic advantage. Lie was an unlikable man: vain, arrogant, and petty. Yet his commitment to peace was admirable even though his performance left much to be desired.

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BRUCE CUMINGS. *The Origins of the Korean War*. Volume 2, *The Roaring of the Cataract, 1947–1950*. (Studies of the East Asian Institute, Columbia University, number 1.) Princeton: Princeton University Press. 1990. Pp. xviii, 957. \$99.50.

This book is the long-awaited sequel to Bruce Cumings's first volume, published in 1981 and subtitled *Liberation and the Emergence of Separate Regimes, 1945–1947*. That work challenged the "official" accounts of the war, touching off a hot debate on its origins. The author had taken up the difficult task of understanding the war against a background of broad historical forces, including Korean internal situations that were the legacy of the colonized Third World, and called the war a nationalist revolutionary war rather than an international struggle between the communist bloc and the democratic world. Volume 2, a mammoth twenty-two-chapter work, picks up where the first volume left off and traces the events of 1947–50, but expands the discussion of U.S. foreign policy formation and global politics at the onset of the Cold War while continuing to explore Korean domestic conditions.

Cumings's second volume contributes greatly to the understanding of the Korean War. The most noteworthy new contributions are the introductory chapter, part I (titled "America"), and the epilogue. In the introduction he lays the groundwork for his method of discourse and theoretical framework for U.S. foreign policy. In part I (chapters 2–5) he elucidates the hidden politics, personal elements, and randomness behind the formation of U.S. foreign policy. Cumings provides new information in the epilogue, in which he narrates events beyond 1950, relating how a war of containment slipped into one of rollback (in October 1950) and then shifted back to containment, and how U.S. bombing of North Korea, so little of which was

reported to the American public, laid the land barren and decimated the population.

Cumings's "digression" on the method of inquiry in the beginning of the book sets this work apart from other historical inquiries and elevates it to a philosophical discourse. He professes that "this book begins, proceeds, and ends with a combination of theory, mystery, old questions answered and new ones left exposed, a text conditioned by its own ignorance" (p. 9). He deliberates on randomness and determinism, comparing political events with snowflakes that simultaneously seem random yet structured.

The author's treatises on the theories of U.S. foreign policy offer refreshing insights and alternative views. He believes such policy results almost exclusively from domestic political struggles—in this case particularly between the departments of State and Defense. The three parameters of internationalism, containment, and rollback and their conflict formed a foreign policy that seemed directionless in the beginning of the Cold War. Also intriguing is the importance Cumings places on human elements in shaping history. He believes that reliance on written documents alone, without the infusion of human dimensions, cannot give true pictures. Cumings quotes Dean Rusk: "Documents are useful but far less so than the daily, ongoing discussions of all sorts, hardly any of them recorded for posterity" (p. 12). The author emphasizes the need to seek out human dynamism within an individual and in human interaction.

Cumings's treatment of the "second" Korean War, the war of rollback, may make this work more controversial than the first volume and demonstrates his courage for intellectual honesty. This phase of the Korean War has been little known and not widely discussed. The list of events is long: the rerun of the conflict between Syngman Rhee and the State Department, the State Department's secret plan to remove Rhee, the undisclosed Rhee-MacArthur plan for occupying and governing the North, the U.S. intelligence groups' collusion with MacArthur and Rhee, and the State Department's inability to put in place mechanisms for supervision of the political aspect of the rollback which resulted in the untold atrocities committed by both South Korean and American soldiers during the northern occupation (pp. 715–21). Very few of these atrocities in 1950–51 were reported, for they occurred in the atmosphere of brutal hot war on the Korean peninsula, at the beginning of intense cold war in Asia, and in the witch-hunt atmosphere of McCarthyism in the United States. In addition, "the overwhelming influence of the United States blanketed critical opinion on a global scale" (p. 642).

Two controversies that Cumings generated in the first volume and that he continues in this work are the nature of the war and responsibility for starting it. The Korean War was not, Cumings argues, a war between communism and democracy but "a civil and

revolutionary war to unify and transform the country" (p. 667). Its starting point was the "liberation in 1945." Partition at the Thirty-Eighth Parallel carried "an immanent energy that violated the unitary and confining entity, Korea." From the time of the partition forward, "the war was predictable if not inevitable." It was "a resultant and denouement mistaken for a beginning" (p. 9).

Cumings presents three "mosaics," or hypotheses, on the question of who started the war. Two are "official" conspiracy theories of the South and North, mirror images of each other. The other is the most plausible scenario, according to the author. After discussing how improbable the conspiracy theories are, the author suggests (p. 568) that the question of responsibility should not be asked, for the following reasons: documentary evidence is sketchy and its authenticity questionable; events converged in the summer of 1950 (a total history concept); someone took a chance (a snowflake phenomenon, a random act); and no good can be served by continuing to ask the question, so all parties concerned should look toward the future (a rational universal law).

Cumings acknowledges that the war of 1950 started when the fighting erupted at Ongjin, probably with the North firing the first shot. But he challenges the established story that the North stealthily prepared for a full-scale unprovoked invasion and that the North might have been misled by Dean Acheson's speech of January 12, 1950, which did not include Korea in the U.S. defense perimeter. Acheson's critics blamed him for being irrational and Acheson faulted himself as "eager and inexperienced" (p. 410). But he was neither, according to Cumings. Acheson was an executive who thought through the logic of a problem (p. 411) and had a configuration of a policy, which Cumings calls "Far Eastern Architecture" (p. 413), a global approach to the East Asian question. Acheson might not have named Korea in his speech but Korea was a part of the "architecture." Besides, the North could not have been misled, for by the spring of 1950 the North had detected, Cumings claims, the shifting dynamics of U.S. global policy, which included winning the war in Indochina. This was called "total diplomacy" by the North (p. 438).

He writes that the firing at Ongjin in June 1950 was more like a reaction by the North to provocation by the South, not dissimilar to the border skirmishes that were almost continuous throughout the summer of 1949. The only difference in 1950 was that both sides were ready to fight. The North especially was in a high state of readiness (p. 594), with the majority of its crack troops returning from China. Thus, Cumings confesses that he is unwilling to commit himself to one interpretation or another and says "it makes little sense to pursue responsibility on who started the war and it is a wrong question to ask" for it "is lost irretrievably to history, unavailable to any retrospective uncovering and act of will." Instead, Koreans

have to learn, the author exhorts, to develop a sense of worldliness and disgust for narrow "fatherlandishness"; they should "love the south in the north and the north in the south" (p. 621).

Cumings has attempted to write a "total history" of the Korean War by looking at causes and effects as "a continuum and a flux," not "in terms of an arbitrary division and dismemberment." A total history concept does not attempt to include everything but instead applies a principle of persistent back-and-forth dialectic between the observed fact and one's theory (p. 7). This is how the author succeeds in giving meaning to the bewildering assortment of data he includes in this volume. He overachieves in this exercise. Occasionally, historical facts are fit into his theory, not theory to facts.

Readers who pick up this thick book dreading being bogged down with detailed historical recapitulation of events preceding the Korean War will be pleasantly surprised by its readability, intellectual flair, multifaceted approach, human dimensions, and the author's empathy with the people and country he writes about.

Yet if readers are looking for simple answers to the origins of the Korean War, they are in for a disappointment. The author provides abundant food for thought but shies away from offering simplistic answers. In fact, Cumings leaves more questions unanswered than answered. The questions that he avers should not be asked are the ones that need to be examined to set the record straight, such as the central issue of who started the war. One is left with an impression that he is less willing to answer them because they do not fit his premises. The North's first shot at Ongjin is a case in point. He delves deeply into the incident, but his explanation of how the fighting spread so rapidly all along the Thirty-Eighth Parallel is less satisfactory.

Cumings's first volume introduced a new and novel theory on the nature and origins of the Korean War and contributed a great deal to the study of post-World War II Korea, East Asia, and the United States. Volume 2 continues this tradition and brings forth a tremendous amount of new information to the multifaceted events that led to the war. But on the origins of the Korean War, the second volume offers little new in support of the author's alternative views, although Cumings demonstrates more openness and less of an author's arrogance and dogmatism.

Francis Parkman viewed history as a public service—to tell the full story. Cumings appears to agree. He asks many questions and attempts to answer them with prodigious research, and yet is candid about not having all the answers. Cumings succeeds in making the "unknown" Korean War a little more known by presenting the other side of the story, but his volumes by themselves do not represent a balanced account. They are, however, indispensable reading for those

wishing to acquire a better understanding of this complex event.

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WILLIAM LAZONICK. *Competitive Advantage on the Shop Floor*. Cambridge: Harvard University Press. 1990. Pp. vi, 419. \$37.50.

William Lazonick presents an ambitious examination of the interactions of capitalists, workers, and technologies in producing "value": high-quality products at low unit costs. Using the industrial economies of Great Britain, the United States, and Japan over the past two centuries, Lazonick analyzes the ways in which different forms of work organization have interacted with technological changes to yield varying economic outcomes. He focuses on the ways in which the structures of social power have shaped work effort, the development and use of production technology, and the distribution of gains from productivity improvements.

In building his theoretical framework, Lazonick attacks Karl Marx for portraying technology as an independent engine of change in capitalism and failing to recognize the power of workers, especially skilled workers, to retain control over the shop floor. Lazonick also rejects classic economic theory, which assumes that impersonal market forces determine wages, input prices, and production costs.

The book's middle section concerns cotton-spinning technologies in Great Britain and the United States in the nineteenth and early twentieth centuries. Technological enthusiasts had predicted that the self-acting mule introduced in the 1830s would replace adult mule spinners with low-wage earning women and children. The replacement workers, the "mind-ers," were young men in their late teens, because the work required strength, stamina, and sufficient "prestige" to supervise the younger "piecers." Minders maintained craft control over spinning, became sub-contractors who recruited and paid the piecers, and established powerful unions in the 1860s. Minders became part of the "labor aristocracy" of the late nineteenth century.

Lazonick compares the capital-labor-technology interface in the cotton-spinning industry in Oldham, Lancashire, with that in Fall River, Massachusetts. British manufacturers cut costs by increasing the use of short-staple, poor-quality cotton, which caused increased breakage of yarn and an intensified work effort, but did not significantly increase labor costs. Productivity and technology stagnated while labor retained control over the shop floor. Manufacturers at Fall River could follow a different strategy because unions were weak there and managers had never surrendered control over work processes. The Fall River manufacturers used better-quality cotton,

which saved expensive labor and yielded superior yarn and in turn enabled them to adopt new technologies, namely the ring spindle and the automatic loom.

The last third of the volume is a broader comparison of British and American industry in the twentieth century, with occasional references to Japan. In Britain, the persistence of craft control over the shop floor, combined with the failure of industry to develop managerial structures capable of introducing labor-saving technologies, account for that nation's serious industrial decline. Unlike their American counterparts, British entrepreneurs failed to replace the market with the "visible hand" of managerial capitalism.

American manufacturers gained control of the shop floor between the 1890s and the 1920s by destroying unions, using technology to de-skill labor, and increasing the supervision of workers to accelerate production. Lazonick argues that American workers benefited from these managerial initiatives through higher incomes and improved job security, an interpretation that bears little resemblance to historical fact. In examining American manufacturing industries after World War II, Lazonick notes their poor performance, especially compared with the Japanese, and suggests that too-powerful American industrial unions are to blame for the dismal results.

Lazonick concludes that productivity gains under capitalism are the result of employer-worker cooperation. When workers have cooperated with employers' efforts to introduce labor-saving technology, workers have shared in the productivity gains. I do not find these conclusions terribly convincing. Lazonick has nevertheless produced an innovative, thought-provoking study of the interaction of capitalists, workers, and technology in modern industry.

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ANCIENT

IAN HODDER. *The Domestication of Europe: Structure and Contingency in Neolithic Societies*. (Social Archaeology.) Cambridge, Mass.: Basil Blackwell. 1990. Pp. x, 331. Cloth \$54.95, paper \$19.95.

This is an extraordinary book, one in the splendid new "Social Archaeology" series. The subject matter is the Neolithic Age, a period arguably the most important in human history. It was the epoch in which southwest Asia and Europe (the latter beginning about 7000–6500 B.C.), among other areas of the world, were settled by villagers who lived on cultivated plants and domesticated animals. Although not Ian Hodder's overt intent, this book gives powerful arguments as to why historians, in their teaching and writing, should pay much more attention than they normally do to this seed time of civilization. Hodder

makes his case in two ways: by a rapid survey of several areas of Europe from the end of the last Ice Age and, much more importantly, by reconceptualizing the human societies that occupied those areas. It is the latter method, focusing on the ideas, that makes this book so important. These ideas derive, in part, from concepts of symbolic anthropology and from modern French schools of thought. Those who work in social history, especially those interested in what we see as conceptual structures in societies, will find Hodder's book a rich harvest of ideas.

When exploring any prehistoric age, all archaeologists have to work with are the physical remains of past lives: settlements, public structures, graves, artifacts. All have symbolic content of one level or another. Contrary to usual archaeological explanation, which tends to be economic, Hodder thinks that these symbols may be the prime means of understanding how and why the Neolithic Age emerged and how societies in it changed over time. The central concept was the domestication of the wild, through a set of ideas he calls the "domus." More than a physical house, it meant home, family units, social structure, the themes of "inside" and "outside" (the latter he calls "agrios"), and life and death. Evidence abounds that the domus long predates agriculture, that cultivating plants and taming animals was not necessarily done from economic necessity, but comes from a set of preexisting ideas. There was, then, no Neolithic revolution in a modern sense of the word, only reinterpretation by peoples of a known and unknown world. In much the same way, the domus lost its centrality—although never all its traditional power—in the later Neolithic Age. The periphery, the agrios, became the center. Led by those who worked in the outside sphere—hunters and warriors—the domus broke up into small residential units that were then reformed into what we recognize as nucleated villages, often with defined boundaries. Along the Atlantic seaboard the new focuses of social interest were public/private monuments, tombs, henges, and stone circles. Human history, in this view, is a process that works through ever-evolving bundles of ideas in the minds of its participants.

This is wonderful stuff, well written from a surprisingly personal point of view. Incidentally, Hodder deals very well with two modern ideological issues in the field: invasion hypotheses versus internal evolution in the explanation of social change, and radical feminist ideas. Although the reader not familiar with European prehistory may find the roster of culture names and sizes puzzling, the concepts will change one's ideas about the prehistoric past.

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P. W. L. WALKER. *Holy City, Holy Places? Christian Attitudes to Jerusalem and the Holy Land in the Fourth Century*. (Oxford Early Christian Studies.) New York:

Clarendon Press of Oxford University Press. 1990. Pp. xviii, 438.

Constantine the Great, the first Christian emperor of Rome, inaugurated a Byzantine millennium with the foundation of Constantinople in A.D. 324. Overnight the ragged and insignificant province of Palestine, now firmly under Christian rule, received undreamt-of largess. Such attention aggravated questions still urgent today: what would be the status of the Holy Land and its religious landmarks, particularly Jerusalem (called Aelia Capitolina by the Romans and then bereft of Jews), in Christian theology, historiography, and politics? How indeed would holiness be defined on the political and religious landscape? In this important book, P. W. L. Walker vigorously reconsiders the two clerics most influential in establishing the character of Christian Palestine, Eusebius of Caesarea (d. 339), and Cyril of Jerusalem (d. 386). The chief contribution of the book is its reinterpretation of Eusebius as a man remarkably constant in his opposition to a theology of holy places centered on Palestine, preferring to emphasize spirituality and the understanding of the divine *Logos*.

The book has three parts. The first, on "problems and personalities" (pp. 3–130), provides biographies of Eusebius and Cyril and introduces their attitudes toward Jerusalem and the sites of Jesus's life. The second part, on "the places of Christ" (pp. 133–308), discusses in greater detail their attitudes toward the Galilee, Bethlehem and the Triad, the Mount of Olives, the Church of the Holy Sepulchre, and Mount Sion. Walker contrasts Cyril's and Eusebius's interpretations of Jerusalem itself in part three, "the city of Jerusalem" (pp. 311–401). He adds an appendix on dating and authenticity, a bibliography, and indexes to scriptural references, texts, and general topics.

Cyril wrote his influential *Catechetical Lectures* only fifteen years after Eusebius's *Life of Constantine*, but the men were products of different worlds. Eusebius was already elderly in 324, while Cyril grew up in the "Peace of the Church"; their understanding of religious geography differed considerably, not least for political reasons. Rivalries separated the churches of Jerusalem and Caesarea, the latter of which held the larger Christian community prior to 324.

Eusebius looked askance at the ambitions of the Jerusalem church. He believed that Jerusalem (like other sites of Jesus's life) had lost its theological significance and retained importance only as a witness to historical events. For Eusebius, the Christian message was universal, not rooted in any particular geography. The Second Coming need not be anticipated in Palestine, for example, nor theophany limited to any place or time. Cyril's theology, on the other hand, accommodated holy places more easily. Unlike Eusebius, he emphasized the Incarnation and believed the historical presence of Jesus made a site holy, for divinity might still be encountered there. As

Bishop of Jerusalem, he felt that his Christian city had always been holy, although distinct from its Jewish antecedent. He promoted Jerusalem and pilgrimage.

A very personal epilogue (pp. 402–05) offers a “Eusebian” solution to the problems of modern Jerusalem. Walker wonders if “Christians [could] diffuse that tension by relinquishing their attachment to the city, by transcending that significance for the sake of Christ, by denying the inherent holiness of Jerusalem,” for “Eusebius’ struggle, indeed his predicament, has become our own” (pp. 404–05).

Walker might well have quoted his sources more extensively, because texts and translations of Eusebius and Cyril will not be easy for all students to find. Readers should keep at hand Timothy Barnes’s *Constantine and Eusebius* (1981) and E. D. Hunt’s *Holy Land Pilgrimage in the Later Roman Empire* (1982) for further background. Throughout the book, Walker summarizes his discussion at the drop of a hat, producing unnecessary repetition, but all in all he deserves thanks for a well-argued and stimulating study.

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ABDULWAHID DHANUN ṬĀHA. *The Muslim Conquest and Settlement of North Africa and Spain*. (Exeter Arabic and Islamic Series, number 3.) New York: Routledge. 1989. Pp. xiv, 280. \$67.50.

The early history of Islam in North Africa and Spain is buried under a mass of legend and misinformation, error compounding pious, or not so pious, invention for centuries after the events. In this book, originally a doctoral thesis at the University of Exeter, Abdulwahid Dhanun Ṭāha attempts to disinter the facts. His principal aim is to sort out the details of the early settlement by Muslims, both Arab and (newly converted) Berber, of the new territories of the extreme west. Despite the promise of the title, the work mainly concerns Spain; North Africa is studied essentially as providing the path to the peninsula and much of the manpower for the conquest.

The first half of the book covers the conquest and the earliest settlements, while the second deals with the period of the governors dependent on North Africa and Damascus, up to the arrival of ‘Abd al-Rahmān I al-Dākhil and the establishment of effective local independence in the peninsula in A.D. 756. The book’s main strength is its collection of data on the nature and distribution of early settlement. Although some of this repeats work carried out by others, especially on Ibero-Islamic social structures and place-names, it is useful to have it all together here, and this is the first such study to appear in English.

Ṭāha is less strong on the interpretive side, however, and this, together with a frustratingly awkward

bibliographical system, makes the book less useful than it might have been. On occasion the author’s judgments appear simplistic; he reports, for example, that at “the sixth Council of Toledo (A.D. 638), the bishops decided that no one was to usurp the throne” (p. 37). The frequent dismissal of political ideology and social and tribal differences as motives for large-scale political actions, or as explanations for great change, in favor of the mere desire for increased material possessions, will not satisfy most students of the subject.

The author is not always consistent in his argument. He dismisses as out of date the old division between Qays and Yaman, which was used so often in the past to explain such problems as tribal rivalries and quarrels in Spain and elsewhere (p. 214). He may be right to do so but much of what he says in the same chapter is based on precisely such an interpretation (see especially pp. 223–24). He reports that one group of Jews was compelled to sell all its property to the state at fixed prices and refrain from engaging in any trade, while another group was conditionally exempt—all that the members of this latter group had to do was to convert to Christianity and give up all their possessions to the dux (p. 43). What the reader is to make of this is difficult to see.

The book also exemplifies one of the perennial problems in the historiography of Islam in Spain, the lack of real communication between Arabists and non-Arabists. The author seems unaware of the real forms, in the languages used by their bearers, of such names as Nikephoros (presented here in its Arabic dress as Neqfur) and seems to believe that in the pre-Islamic period many places were called by the later Arabic corruptions of their names (for instance, Ibāriya for Iberia and Qartājinnā, for Cartagena). But the book makes one contribution of the utmost usefulness: its frequent indication of the existence of mines and other sources of salt in al-Andalus. This is an important topic, scarcely touched on in works on al-Andalus, and it is to be hoped that Ṭāha will turn to this subject in the future.

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RICHARD SORABJI. *Matter, Space, and Motion: Theories in Antiquity and Their Sequel*. Ithaca: Cornell University Press. 1988. Pp. x, 377. \$49.50.

This book is the final installment in Richard Sorabji’s great trilogy of studies in Greek physics and metaphysics. Readers familiar with the first two volumes (*Necessity, Cause, and Blame* [1980]; *Time, Creation, and the Continuum* [1982]) will expect a very high standard both of scholarship and of philosophical interpretation. They will not be disappointed. Reviewers of the earlier works invariably praised the richness and breadth of Sorabji’s learning. His new book ranges

equally (indeed perhaps more) widely in philosophy and science—ancient, medieval, and modern—and evinces an admirable and wholly unusual grasp of all aspects of that complex, tortuous, and exciting history.

Sorabji's subjects are the rival theories of the construction of the cosmos, its constituents, and their dynamics. He draws on pre-Socratic theory and accords Plato's *Timaeus* its due, but it is Aristotle who here, as in the preceding volumes, is the pivotal figure. Sorabji sees the development of science and cosmology in the ancient and early medieval world as an extended conversation between the proponents of competing pictures of the structure of the cosmos. He is at pains to emphasize the continuity of the dialogue and to show in detail where it is plausible to interpret one theorist as reacting to the views of another. Central to the establishment of this dialectical picture is the rejection of the view, most associated with F. M. Sandbach, that the Stoics developed their cosmological views largely in ignorance of, and hence in isolation from, those of Aristotle and the Peripatetics. Scholars may dispute the extent and pervasiveness of that influence, but no one will now, I think, maintain that there was none.

One advantage of viewing history in this dialectical manner is that it provides the scholar with a set of powerful interpretive tools: the obscure passages may be rendered less so by reflecting on what problems created by their predecessors they were plausibly intended to solve. Such an approach requires charity; to think that an author is merely confused, or simply stupid, is a recourse of last resort. Sorabji's work abundantly demonstrates the historical and philosophical advantages to be won from such an approach.

Sorabji's work is, and should be read as, philosophy in its own right. He frequently digresses to the views of the moderns in order to underline points of contact and contrast with the principal objects of his study—but that is not his only concern. He also seeks to develop and defend still-current philosophical views (that circular or closed time is possible, for example, or that backward causation is coherent). These views grow out of discussions of the ancient subjects and are conditioned by Sorabji's evident admiration for their strength and power. But they are outgrowths in their own right: history is not made to substitute for philosophy. Neither, however, are the interpretations foisted uncomfortably on recalcitrant ancient material under the imperatives of some modern theory; Sorabji's history is genuine. In short, this book and its predecessors are a paradigm of how ancient philosophy should be done, and as a result their appeal is interdisciplinary in the best sense of that much-maligned term.

People have long been inclined to think of Aristotle's cosmology and dynamics as a dead weight on the intellectual world, suffocating the possibility of genuine scientific advancement in the treatment of mass,

matter, and dynamics for almost two millennia. Sorabji's careful and subtle reconstruction of the intense intellectual debate among Aristotelians, Stoics, and Neoplatonists, carried on by philosophers in both the Christian West and the Muslim Arabic world, helps dispel that pervasive myth. This emerges most notably (here those familiar with Sorabji's work will not be surprised) in the treatment of the sixth-century Christian philosopher John Philoponus. Philoponus was a powerful and consistent critic of Aristotle's dynamics, his concept of matter, his account of God's role in the universe, and much else; he was responsible for the first detailed and comprehensive impetus theory. His influence can be traced through the middle ages to the work of Joannes Buridan and others in the fourteenth century. The work of Sorabji, among others, has justifiably restored this neglected figure to his rightful place.

This book is beautifully produced, with detailed and comprehensive indexes; it is written with fluency, humor, and charm. In every way it is an unmitigated delight and will be read with profit and pleasure by anyone curious about the long evolution of physical theory.

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GIULIA SISSA. *Greek Virginity*. Translated by ARTHUR GOLDHAMMER. (Revealing Antiquity, number 3.) Cambridge: Harvard University Press. 1990. Pp. 240. \$25.00.

Giulia Sissa defines the Greek maiden (*parthenos*) as a creature in transition. Although still her father's child, she was, at the same time, approaching the social and physical maturity that would transform her into her husband's publicly wedded wife and the mother of his legitimate children. The *parthenos* was doubly marginal, only a potential member of the race of mature womankind that was itself liminal in the public life of the anthropocentric city-states of classical Greece.

The book consists of three essays, in the first of which ("The Enigmatic Virgin") Sissa argues that the special qualities of the Greek virgin were essential to the prophesying of the Pythia, priestess of Apollo at Delphi. Inspirational breath entered through her lower, genital mouth, possessing her, while her upper mouth remained closed in maidenly modesty to retain the god's secrets. In the second essay ("The Virginal Body"), Sissa contends that male penetration did not terminate Greek virginity and that the *parthenos* retained her status provided that the consequences of a clandestine union, pregnancy and birth, were known to her alone. Greco-Roman science never isolated the hymen that modern anatomy identifies as occupying the vaginal introitus, and thus no visible bodily part defined virginity lost. The third essay

("Neither Virgins nor Mothers") explicates the Danaides as emblematic of females caught in the moment of transition, because they murdered their husbands on their wedding night. Archaic Greece early codified the image of the postmenarchic female body as a leaky vessel: insatiable, yet also ever-wasteful of its contents. As punishment for their crimes, mythology assigned the Danaides the never-ending task of filling bottomless jars with water carried in sieves. Their bodies never closed in the conception of a child, when the mouth of the uterus was thought to shut, retaining seed and menses in a pregnancy that both filled the female body and fulfilled woman's societal role.

This book is a stimulating work that ranges widely over the ancient evidence, combining it in provocative ways and unequivocally demonstrating that virginity is a cultural construct. While Sissa is right to insist that Greek virginity lacks a hymen of defloration, however, there is evidence in Greek and Roman writers for a folk anatomy that imagined an unseen closing at the mouth of a young girl's uterus, obstructing menses until they broke through at menarche or were liberated by first coitus, whichever came first. An earlier version of her second essay (a translation appears in David Halperine *et al.*, *Before Sexuality* [1990]) likewise conflated vaginal and uterine mouths, yet it better confronted contrary evidence, especially the Hippocratic treatise *Young Girls* (wrongly cited as *Diseases of Women* on p. 121), which describes madness in prepubertal girls as caused by blood retained prior to menarche and prescribes intercourse as cure. In the present version, Sissa supposes that prior to menopause the mature female body, a leaky vessel, could resume an unwedded, infantile state (pp. 122–23), an assumption as unhealthy for Hippocratic writers as it was unacceptable to their society.

Those who refer students to this book should be aware of confusing inaccuracies (for example, p. 45, read "widows widowed when young and already mothers"; p. 66, where *hystera* is glossed by "rear"; pp. 101–02, where the first passage from Pausanias lacks reference to the father's discovery and makes Nauplius his agent in drowning Auge, while the second omits "in a bed").

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ROGER JUST. *Women in Athenian Law and Life*. New York: Routledge. 1989. Pp. x, 317. \$35.00.

RAPHAEL SEALEY. *Women and Law in Classical Greece*. Chapel Hill: University of North Carolina Press. 1990. Pp. viii, 202. Cloth \$24.95, paper \$10.95.

Apart from the words "women" and "law," the most striking common feature of these two books is that both were conceived in the mid-1970s, when women's studies in the classical field was just getting off the

ground, but have been published only in the last few years, when discussions of gender-related issues are both numerous and increasingly sophisticated. That both books may strike a reader today as somewhat stale—as yet two more forced marches through the well-trampled field of Greek men's view of women—is in fact an indication of the remarkable growth of ancient gender studies in the last fifteen years.

In particular, it is the misfortune of these two books to appear at the very moment when John Winkler's *Constraints of Desire* (1990) has significantly redrawn the boundaries of discussion with readings of the *Odyssey*, Sappho's poems, Longus's *Daphnis and Chloe*, and Athenian public religion, which stretch a reader's understanding beyond the androcentric perspective in "an attempt to restore the circumscribed dignity and autonomy of Greek women, undervalued both by ancient Greek men in their public pronouncements and by victimhood theorists in modern times" (p. 12). In essence, both Roger Just and Raphael Sealey present in their books an inherently restricted male view of Greek women (whether in law, literature, religion, or philosophy).

Although Sealey and Just range far beyond the scope of Greek law itself (Sealey in his chapters "Women in the Roman Republic" and "Women of Homer," Just in his two final chapters, "The Enemy Within" and "The Savage Without," dealing primarily with Athenian drama and the "nature" of women), the legal status of Greek women is a common concern for both authors. The traditional questions of marriage, legitimacy, and inheritance—questions about what constituted legal marriage or what the intent was of laws regulating the marriages of heiresses—are the main legal fare of both books.

Just is the more thorough in his discussion (pages 40–104 concern these topics); in particular his clear presentation of the structure of Athenian inheritance (pp. 83–95) is most welcome. But the argument is at times careless in detail (for example, on the status and identity of the *pallake* [concubine]) and overdependent on a few past discussions, such as John Gould's article in *The Journal of Hellenic Studies* (1980). Furthermore, overreliance on some standard discussions is accompanied by a disregard of recent works, particularly those relevant to the final chapters of his book. Just defends this approach with his prefatory comment, "with regard to secondary sources I have been highly selective, citing only those that I believe to be the most authoritative, or with which I most agree" (p. viii; see also his general disclaimer on ix), but this hardly explains the conspicuous neglect of the publications of—to cite only a few—Froma Zeitlin, Helene Foley, David Cohen, Page duBois, or Lesley Dean-Jones over the past decade and a half. Bibliographical weakness aside, however, a reader expecting a new or methodologically innovative approach from an author who writes as a "social anthropologist rather than an ancient historian or a classicist" (p. viii) will find Just's book disappointing.

Sealey appears to be the more original author (the chapter on "Women in the Laws of Gortyn" is especially helpful), but his text suffers from idiosyncrasy and—like Just's—a traditional androcentrism that sees woman as "the other": as passive, or as the object of male culture. Sealey repeats his previously published views on the nature of Athenian marriage (*Classical Antiquity* 3 [1984], 111–13; *The Athenian Republic* [1987]), emphasizing the passive role of the woman and the lack of legal concern for the actual nature of the union. He contrasts this to Roman marriage and Roman family structure where, interestingly, he contends that the "extensive authority" of the paterfamilias "was a source of equality between the sexes" (p. 158). Sealey presents his views with a breezy confidence, but they are hardly uncontroversial.

Even more, Sealey's interpretation is limited by a perspective that sees Athenian (and in general probably Greek; see the last chapter on "Women and the Unity of Greek Law") marriage as being essentially the transaction of the *engue*, by which one man entrusted a woman to another man for the purpose of producing legitimate heirs. Sealey insists throughout the book on the passivity of the Greek woman, from the faithful Penelope (would that he had read Winkler) to the adulterous wife of the speaker of Lysias 1. And, as with Just, there is a noticeable tone of arrogance in the dismissal or ignoring of current feminist work in both Greek literature and history (see especially Sealey's Appendix B).

Just claims that his book, covering "a broad range of evidence concerning women in classical Athens," fills an "obvious gap" in the literature (p. viii). That gap may be less evident in 1991 than it was when both Sealey and Just began to write. In any case, it is certainly an open question whether there is now real need for another routine discussion of the crime of Clytemnestra (Sealey, pp. 140–44) or for comments such as the claim that, "given the high degree of enforced idleness in which upper-class Athenian women lived," the charge of gluttony may not be "without empirical foundation" (Just, p. 185). Just carefully qualifies that last statement by noting that Greek portrayal of women as gluttons had more to do with a conception of the "naturally 'unfree' female" (p. 185) than with actual conduct. It is a weakness, however, of both of these books that they do not go beyond rehearsing that well-known conception once again.

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MICHAEL STAHL. *Aristokraten und Tyrannen im archaischen Athen: Untersuchungen zur Überlieferung, zur Sozialstruktur und zur Entstehung des Staates*. Stuttgart: Franz Steiner. 1987. Pp. xiii, 287. DM 78.

The emergence and role of the aristocracy, the development of the polis and of the state, and the role of tyrants are certainly among the most important and challenging issues concerning the history of archaic Greece. Research on these issues has been intense in recent years. Generally, however, work in the field is hampered by the lack of adequate and specific sources. Archaeology continues to contribute much, but in limited ways; Homer and the lyric poets did not write for historians and thus rarely provide direct answers to our questions. Even Herodotus leaves us with an abundance of difficulties: he based his historical reconstruction mostly on oral traditions, depended on the selective and far from impartial memory of his informants, and selected and shaped his narrative according to his own priorities and purposes. Thus, historians working on archaic Greece have to rely extensively on archaeology; they will profit as well from applying anthropological approaches and the theoretical frameworks suggested by the social sciences for the study of early societies; and a solid methodological foundation must be created for the interpretation of literary sources, taking into account not only the characteristics, potential of information, and limitations of each genre but also the nature of, and problems with, the oral traditions underlying all of them.

In all these respects, Michael Stahl's book is exemplary. It focuses on those episodes in sixth-century Athenian history that are illuminated by some of Herodotus's best-known digressions (on Peisistratus and his sons, the family of Miltiades, and the Alcmeonids), but for which there exists other information (literary and archaeological) as well. Stahl's investigation is based on two crucial assumptions: first, the question of the nature of tyrannical rule cannot be separated from that of the structure and development of aristocracy and the state in archaic Greece; second, none of these questions can be answered reliably or even adequately without a solid understanding of what our sources can, and cannot, tell us and how they can be interpreted most fruitfully.

Accordingly, the first section of the book, "Oral Tradition and History" (pp. 6–53), is devoted to a systematic discussion of the complex set of problems revolving around the use made of oral traditions by Herodotus. Stahl distinguishes between structural and other narrative elements, tries to determine the purpose of each oral story in its interpretive context, and separates marked components of stories from the less marked. As a result, Stahl is able to identify a core of information that is likely to be relatively unaffected by the changes naturally occurring in repeated recountings of oral traditions and can thus be used to reconstruct not so much the events but the structure of sixth-century social relations and politics. The second section, "Stasis and Tyranny in Archaic Athens" (pp. 56–136), is divided into two parts. The first analyzes the traditions about the feuds between three factions in Athens (Her. I, 59–64) in order to grasp

the characteristics and to establish a typology of aristocratic life, relations, and politics. The second part of the section uses the stories about Miltiades the Elder and Younger, Cimon the Elder, and the Alcmeonids to examine the changing relations between aristocrats and tyrants.

The third section, "Tyranny and the Emergence of the State in Athens" (pp. 138–255), reviews in general and theoretical terms the question of the formation of the state in antiquity and then focuses on four areas (officials and law courts, laws and citizenship, foreign policy, and communal integration and identity) to analyze the emergence and development of state structures before and during the tyranny of the Peisistratids. While the tyrants served their own purposes of securing power and rule, they in fact enhanced state structures, creating the strong foundations that enabled Cleisthenes to succeed with his reforms. Thus, tyranny, while remaining fully integrated in the structures of aristocratic society and exploiting to its own advantage all the forms typical of aristocratic interaction, at the same time strengthened communal structures, integration, and identity and thus created indispensable premises for the later development toward democracy.

This is a dense and rich book, full of important considerations and observations. Its results are largely convincing. It is well organized and clearly argued; methodologically conscious, explicit, and, for the most part, sound; and well informed about current international scholarship. Its scope is broad, frequently connecting the developments of the sixth century with those of earlier archaic and later classical history as well as archaic Rome. Disagreement is possible at various points, but hardly affects the overall picture Stahl presents. With this book Stahl has considerably advanced our understanding and created a new and better foundation for future discussion of aristocracy, tyranny, and state in sixth-century Athens and, more generally, in archaic Greece.

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DONALD ENGELS. *Roman Corinth: An Alternative Model for the Classical City*. Chicago: University of Chicago Press. 1990. Pp. 264. \$34.95.

As the subtitle of this book suggests, this is not a straightforward history of Roman Corinth. Donald Engels instead uses the available evidence to flesh out a new theoretical model meant to elucidate the core features of the classical city. For the purposes of this study, then, Corinth is merely an exemplar.

Engels dismisses at the outset what he aptly labels "the primitivist, consumer-city paradigm" (p. 4). Most fully articulated by M. I. Finley in *The Ancient Economy* (1985), this widely endorsed construct holds that the classical economy was characterized by subsistence

cultivation and an underdeveloped urban marketplace; hence the classical city had to subsist parasitically on the taxes and rents generated by peasants in the surrounding countryside. Engels demurs with his own notion of the "service city." The classical city flourished, he claims, in part because its market offered needed goods and services to visitors, but above all because the peasantry routinely produced a surplus fully 30 to 50 percent in excess of household needs, which it promptly spent in the marketplace (pp. 41, 125, and especially 127–28).

Ancient historians badly need theoretical alternatives to Finley's often rigid and simplistic views, and it is especially refreshing to see the relationship between town and country described in symbiotic rather than adversarial terms. Thus, it is all the more to be regretted that Engels's model is as rigid, simplistic, and unconvincing as the one he attacks. The choice of Corinth for such a study is unfortunate. Cicero (*De Republica* 2.7–8) and Strabo (8.6.20) both link Corinth's prosperity to its commerce; Engels rightly endorses this view of the Corinthian economy (p. 213, n. 64), but fails to see its implications. Corinth could never appropriately be called a "consumer city." But is it in any sense a typical Roman community? Engels contrasts it with Athens, Thessalonica, Philippi, Mantinea, and Plataea (pp. 113–14, 129): does the well-known dependence of Athens on the philanthropy of Herodes Atticus make it more or less representative than Corinth? Is Roman Athens a "consumer city," or must we generate still another model to account for its historical development?

The idea of the "service city" itself rests squarely on the presumption of a large peasant surplus, but it is most unlikely that so idiosyncratic a notion will attract a large following. Engels repeatedly claims that the classical peasant must have had a large surplus because his taxes were tripled in the late empire (pp. 40–41, 133). The mere imposition of a tax, however, says nothing about the capacity of the targeted group to pay it—and in this case, as A. H. M. Jones has made abundantly clear in *The Later Roman Empire* (1964), imperial policy was singularly infelicitous. Engels would have been better served had he concentrated on evidence from the early empire: several of the younger Pliny's *Letters* concern Italian tenants unable to pay their rent (3.19, 7.30, 9.15, 9.37, 10.8), while *The Golden Ass* by Apuleius vividly describes both the poverty and class antagonism that marked the Greek countryside in this period (9.32–38).

Finally, it should be noted that this work suffers badly from anachronism. For example, by our standards medical care was undoubtedly better in the classical city than the countryside, and Engels unhesitatingly cites this among the services that peasants were able to purchase with their surplus (p. 47). There are many sources, however, that suggest that folk medicine never surrendered its popularity even among the elite (Pliny, *Natural History*, 28.28–29), so this assumption is unwarranted. Those familiar with

Apuleius or with the Euboeic discourse of Dio Chrysostomus will similarly doubt whether peasants were prepared to litigate in urban courts or send their children to urban schools—yet it is claimed that these two institutions also absorbed the countryside's surplus wealth (pp. 44–45). Such missteps vitiate Engels's portrait of Roman Corinth, but more importantly they fatally undermine the whole concept of the "service city."

Although it is unlikely that any single model can do justice to the rich complexity of the classical world, it is still to be hoped that more studies of this type will appear in the near future. F. Millar's superlative "The World of the Golden Ass" (*Journal of Roman Studies* 71 [1981], 63–75), which avoids the various failings recounted here, introduces one promising approach worthy of more elaborate treatment.

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THOMAS N. MITCHELL. *Cicero, the Senior Statesman*. New Haven: Yale University Press. 1991. Pp. x, 345. \$32.50.

Do we need another biography of Cicero? With this work, Thomas N. Mitchell completes his two-volume study of Cicero's political career, taking the story from the end of his consulship in 63 B.C. to his death in 43 B.C. The judgments are, on the whole, sane and reasonable, provoking few serious quarrels. Mitchell has contributed a number of valuable articles on late-Republican politics and a fine first volume, *Cicero, the Ascending Years* (1979). But what is offered here that cannot be found elsewhere?

The preface claims that this book combines a presentation of Cicero's career with an exposition of the political ideas and theories that helped to shape it. Well, not really. The discussion of Cicero's political treatises and the philosophy contained therein is concentrated in the first chapter—and rarely reappears to illuminate his actions and decisions thereafter. And that first chapter has its own problems. By drawing out concepts that receive expression in a variety of texts composed over a period of three decades, Mitchell gives little sense of changes in Cicero's thinking, ignores his inconsistencies, and does not consider the extent to which the orator's ideas may have been unrepresentative of current attitudes.

Mitchell provides a lucid and sensible narrative, thoroughly documented. (The notes as guides to the relevant texts constitute an extremely valuable resource.) But telling the tale almost seems an end in itself, with analysis soft-pedaled. Mitchell sometimes presents little more than summary and paraphrase of the evidence, as on Cicero's actions during the civil war (pp. 252–66) or on the delivery of the *Philippics* (pp. 301–16). The vicissitudes of Cicero's life and

times raise numerous questions that frequently go unanswered. To mention just a few: why the swift change in Cicero's fortunes in late 63 and 62 B.C. (pp. 65–73)? Why should the dynasts want him removed in 58 B.C. (pp. 129–37)? What turned the tide for Cicero's recall from exile (pp. 144–57)? How does one account for his contradictory views on Roman imperialism (pp. 205–11)? We look for more interpretation than we get.

A few welcome innovations do emerge. Mitchell perceives the wider context and implications of Nepos's actions in late 63 B.C. (pp. 66–73). He cogently undermines the *communis opinio* that Cicero's actions prompted the triumvirs to arrange for Clodius's adoption (pp. 114–20). And he successfully diminishes the part played by the *lex Campana* in bringing about the conference of Luca (pp. 169–78).

Other suggestions are more problematic. The idea that Clodius acted in concert with the dynasts in 58 B.C. is based on flimsy evidence and reasoning (pp. 130–34). Minimizing Cicero's role in the preliminaries to Luca fails to explain Caesar's anger (pp. 171–73, 181). And can one really characterize Cicero's raising of war against Antony as the act of a pragmatist (p. 311)?

Although Mitchell acknowledges Cicero's weaknesses and faults, he strains to vindicate his hero. The verdict can be defended, but the defense rests heavily on an interested witness. Mitchell too often buys Cicero's own presentation of motives and accomplishments, as in the account of the Cilician proconsulship and even the skirmish with tribes in the Amanus mountains (pp. 219–29). A little more skepticism might be in order.

Discussion of Cicero's personal and family life is skimpy. Piecemeal items receive mention in passing, and only when they crop up in the chronological narrative; they get no treatment in their own right. Mitchell devotes more space, for instance, to the Rechtsfrage between Caesar and Pompey (pp. 236–43), in which Cicero had no direct involvement, than to the concurrent turmoil in his household (pp. 232–34).

One persistent thread runs through the narrative: Cicero's disappointment with and bitterness toward the conservative oligarchy. Mitchell recognizes its importance. But how does one reconcile this with the conservative political philosophy espoused in his theoretical treatises and explored at length in the first chapter? That matter challenges ingenuity. It is not picked up.

Mitchell's study is competent and level-headed, a respectable addition to the Cicero shelf. May we now declare a moratorium on biographies of that individual?

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ROBIN MACPHERSON. *Rome in Involution: Cassiodorus' Variae in Their Literary and Historical Setting*. (Seria Filologia klasyczna, number 14.) Poznań: Wydawnictwo Naukowe Uniwersytetu im. Adama Mickiewicza. 1989. Pp. 367.

Cassiodorus was successively a high official in Ravenna, a refugee in Justinian's Constantinople, and headmaster of Vivarium, the scholarly monastery he founded on his Calabrian lands; he may have lived to be ninety. The *Variae*, or "Assorted Letters," documenting his decades in government from A.D. 507 to 538 are a major monument of late Latin and our richest source about the great Ostrogoth, Theodoric, and his Italian kingdom, which Cassiodorus served for almost as long as it lasted. Robin Macpherson distributes his attention very unevenly between the *Variae* and their "literary and historical setting"; out of 200-odd pages of text, fewer than a quarter concern the letters (even the edition published in 1973 by Å. J. Fridh goes unnoticed). The larger portion of the book is an essay on late Latin antiquity, much like books by Samuel Dill, Eleanor Shipley Duckett, and Nora Chadwick, and rarely surpassing them.

Macpherson evokes a "diseased society" and treats it with aversion (p. 28). He assembles quotations that, in his view, unequivocally prove a contemporary certainty of decline. The Augustan Age and late Republic are his standard of comparison; although skilled in analyzing the literary language of Late Antiquity, he has nothing good to say about its departures from the exemplary past. Better versed in language than history, he equates the imperial army toward A.D. 400 with "the disciplined valour of the legions" and lumps into a "German" collectivity many peoples, notably the Goths, who would have spurned the name (pp. 47, 85–91, 114–15). His belief that western lands were "ineluctably gravitating towards seignory" suggests how summarily he interprets social and economic evidence (p. 230).

The pages on Cassiodorus and his age are more professional. Key dates, however, are shaky: that of Cassiodorus's lost *Gothic History* is affirmed with off-hand brevity; the year and aim of the *Variae* need fuller discussion; omitted facts undermine the origins alleged for Jordanes's *Getica*. Macpherson thinks himself entitled to disregard the secondary literature except for "overall guidance" (p. 8), whatever that may be, and he claims gratuitously that Theodoric's kingdom was "doomed from the start" (p. 229). Nevertheless, he convincingly analyzes the politics of the Ostrogothic period and, in a set of individual examples—the best pages of the book—studies the *Variae* with genuine understanding. Noteworthy comments are made about Ennodius of Pavia, another long-winded letter writer and admirer of Theodoric, as well as about the crisis that cost Boëthius his life and gave Cassiodorus a ministerial promotion. By comparison with his flawed account of the "setting," Macpherson's presentation of Cassiodorus and the

Roman dimension of the Ostrogothic kingdom is important and readable, a very good article swollen into a weak book.

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MEDIEVAL

LEOPOLD GENICOT. *Rural Communities in the Medieval West*. (Johns Hopkins Symposia in Comparative History.) Baltimore: Johns Hopkins University Press. 1990. Pp. x, 185. \$30.00.

In this slim but dense volume Leopold Genicot reflects on fifty years of masterful scholarly research. The book draws on both his close study of the nobility and peasantry of Belgium and his extensive reading of local studies throughout the Continent and England, providing a rare opportunity to compare developments in widely divergent specialties. Bibliographical references range from Austria to Great Britain and Italy to the Netherlands. Based on lectures delivered at Johns Hopkins University in 1986–87, this book emphasizes "problems and methods," raises questions, and suggests directions for future research. The author declines, however, to "propose or impose, solutions" (p. 11).

Genicot begins by discussing the shifting use of the term *villa* in early medieval records to the later use of the term *universitas*, which he regards as indicative of progress toward the creation of community, defined as individuals who are "conscious of being a distinct group" (p. 60). The significance he finds in that semantic shift illustrates well his approach to the problems of definition evoked by the title of his book. He finds in the early Middle Ages the stimuli for the development of a community "qualified to act in juridical affairs, like making binding agreements, buying, and appearing in a court" (p. 31). Genicot wants to trace the progress of men and women as they move along "the path to a real community" (p. 28). In pursuit of that goal, he divides the book into chapters that discuss this evolution in terms of the economic/ecological forces, political/legal forces, and religious practices that contributed to the development of community. In the final chapter he points out the various ways in which these communities were associated into a broader context through trade links with towns, mobility, and participation in political and judicial institutions developed by the church and state. Some rural collectivities even participated in national parliaments (p. 117).

The developmental approach allows Genicot to raise some interesting issues while focusing attention on the vitality and variety of rural institutions. For example, as he traces the balance of powers that evolve between members of rural communities, their leaders, and their civil and ecclesiastical overlords, Genicot suggests that the term "oppression" becomes anachronistic (p. 123). Exploring the *universitas* also

allows the author to raise questions about the functioning of rural assemblies: How often did they meet? Were they representative? What factors within the community were divisive and which unifying? In the discussion of the parish, he recognizes the often important role that the laity played in choosing not only their church wardens but also, at times, their local clergy. In some localities he suggests that building a church or engaging a priest was the "first collective enterprise" (p. 105).

The problems of definition remain stubborn, however, and may hamper attempts to visualize medieval rural communities as entities that evolve from non-community. Genicot also points out that communities are characterized by "territory, mutual assistance, rights and duties, and self-consciousness" (p. 31); and that they share "consciousness of constituting a distinct entity" (p. 38), wherein is "nourished a collective conscience" (p. 23). Perhaps community, in this sense, changes and reshapes itself regularly and is never truly absent, whether expressed within the context of the tribe, hamlet, village, or state.

There are also puzzling questions that remain concerning the relationship between rural communities of peasants and their lords. Genicot's assumption of seignorial initiative in many of the processes affecting the evolution of peasant communities allows him to confront important issues. For example, did the withdrawal of gentry from villages after the Black Death and the increase in the number of outsiders as landlords result in a deterioration of the "social climate" (p. 54)? Certainly rural communities did define themselves, at least in part, as a result of their interactions with lords and officials representing the state. But there are times when Genicot resorts to a "top-down" historiography that betrays much of his interesting material. In parts of his book the peasantry are demoted to the status of "the dominated," which has usually been a code word for the realm of the unknown. In spite of persistent evidence of change, peasants are described as stubborn opponents of novelty (p. 80). And, while citing evidence that villages lent money to individuals or that peasants were involved in debt transactions among themselves, he assumes that peasants' creditors were usually their lords and that their borrowed capital was rarely used for investment, but rather "to make ends meet" (p. 48). Similarly, the author questions whether the majority of the peasantry could have entered into regular connections with markets because "they had so little to sell" (p. 49).

In the end, this volume makes a major contribution to the growing discussion of peasant culture for two reasons. First, it provides a valuable overview of work that has been done in the field; second, the author emphasizes the diversity that results from the complexity of rural life. He points out that one scholar has isolated at least seventeen natural and social variables affecting peasant communities (p. 120). Genicot is wise, therefore, to make an effort to avoid

generalizations and to focus on the myriad of interesting problems that remain to be solved.

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THOMAS HEAD. *Hagiography and the Cult of Saints: The Diocese of Orléans, 800–1200*. (Cambridge Studies in Medieval Life and Thought, series 4, number 14.) New York: Cambridge University Press. 1990. Pp. xvii, 342. \$59.50.

In 871 Bishop Walter of Orléans spoke of the "pious, local patronage" of the "fathers" of his diocese. He meant saints such as Anianus, Benedict, Evurtius, or Maximinus who had founded major churches or monasteries or whose relics had long resided in the Orléannais. Thomas Head takes his lead from Bishop Walter and studies the role of these saints in local society and religion from the ninth century to the twelfth. In a beautifully written study of one exemplary region, Head is able to illuminate many historical issues that are central to current investigations.

Head argues that in the Carolingian period hagiographical texts took a particular form that was rigorously historical: for a church to be taken seriously it had to have a "founding father" (no mothers here, although elsewhere St. Foy or St. Gertrude filled that role) whose history was known in some detail. Detail, historical specificity, secured the support and protection of the church by kings and nobles, bound the religious community together as a group of servitors of the saint, and tied the church tightly to its surrounding society. As the Carolingian state began to decline, lives began to be supplemented or even replaced by narrations of posthumous miracles performed by the saint's relics. Of course, these miracles served to strengthen the bonds between the church and its neighbors but it is how they did so that is interesting. Head speaks of a "logic" that took contractual form. The saints performed miracles to protect the property of the churches as long as they were paid due cult and reverence. In the late tenth century a new twist appeared. Writers, beginning with Abbo of Fleury, tried to complement the local "fathers" with papal and royal patronage, even if they had to forge documents to do so. Then, from the mid-eleventh to the early twelfth century, a final development emerged in the history of local saintly patronage. Miracle stories were no longer written to secure the material possessions of the church. Now, when a church's rights were threatened, it was likely to go to law to secure its interests. Hagiography, meanwhile, recorded numerous miracles of healing experienced by individuals and also began to show an interest in contemporary holy men instead of focusing exclusively on the ancient spiritual heroes of the diocese.

This broad sketch of Head's major findings cannot really do justice to the richness and complexity of his book. The research is truly impressive, encompassing

published and manuscript sources and an extremely thoughtful handling of modern scholarship. Small but important points are made again and again. The liturgical uses of hagiographical texts are intelligently canvassed. Letaldus of Micy and Helgaud of Fleury turn out to have been more interesting than anyone would have imagined and Head finds new things to say about Abbo of Fleury despite the currently flourishing Abbo industry. If there is any life left in the old idea that one can usefully oppose lay and clerical values and conduct, then Head has killed it. The book effectively discusses patronage, community, and literacy. On literacy, Head draws effectively on Brian Stock's idea of "textual communities" to show how written works about saints could stand at the center of a social circle, only a few of whose members could or did read them. For all its contributions, I found myself disappointed on only three counts. First, I am not sure that, as he actually settles in to telling his story, Head is able to follow his own chronology in terms of the major changes he charts. Second, while there undoubtedly was a change in the twelfth century away from old and toward new holy men, there were many spiritual changes in that potentially charged atmosphere and I would have appreciated Head's thoughts on how hagiography might be said to have reflected a wide range of contemporary concerns. Finally, the evidence never really lets Head get laypeople into his networks of patronage as securely and visibly as all of us would like.

But I do not wish to end on a negative note. This is one of the most stimulating books I have read in a long time. It constantly made me think of possible connections between one thing and another. For instance, Head's observations on Carolingian hagiography make imperative a full study of that era's ideas about the past. The transformation of miracle accounts from corporate ones involving churches to personal ones involving the healing of individuals plays into the long-standing discussion of "the discovery of the individual." Might this same transformation from powerful institutions to wounded individuals be a further reflection of that spirituality that saw a change, in depictions of the Crucifixion, from erect, regnal Christ figures to broken, suffering ones? You just cannot read two pages of this book without thinking about such things.

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PENNY J. COLE. *The Preaching of the Crusades to the Holy Land, 1095–1270*. (Medieval Academy Books, number 98.) Cambridge, Mass.: Medieval Academy of America. 1991. Pp. xiv, 281. \$35.00.

Historians have been writing accounts of the Crusades for almost nine hundred years and from the outset they have known that preachers were the principal recruiters of Crusading expeditions. It is

impossible to understand how the Crusades worked—or why and how they ceased to work—without knowing quite a lot about Crusade preachers and how they conducted their business. Yet Penny J. Cole's book is the first study to bring the roles and functions of Crusade preachers, as well as the style and content of Crusade sermons, sharply into focus. Her pioneering monograph should attract the close attention of everyone interested in the Crusading phenomenon, or more generally in the history of political and religious propaganda.

Cole bases much of her study on the two staple sources of Crusading history: the accounts of Crusading chroniclers, who often incorporated extensive descriptions, summaries, or reconstructions of sermons into their narratives; and papal letters, which contain numerous commissions for crusade preachers, sometimes with directions about how they were to proceed with their work. In addition to these standard sources, Cole has explored two other kinds of evidence that previously have been scarcely studied at all: homiletical manuals for preachers, and the texts of Crusade sermons themselves. These sources contribute much that is new and interesting to her account. Particularly valuable are the transcriptions of five thirteenth-century Crusade sermons, which appear as appendices to her book.

Cole finds that the chroniclers' accounts of Pope Urban II's sermon at Clermont fundamentally shaped the subsequent development of Crusade homilies. Reports of Urban's discourse furnished a model that Crusade preachers sought to emulate and that later preachers returned to time and again. Neither medieval Crusade preachers nor modern historians who study them, of course, can know more than approximately what Urban actually said at Clermont; we must depend on hearsay evidence in the accounts of chroniclers, many of whom were monks and few of whom were actually present at the scene that they report. Nevertheless, these accounts gave generations of preachers a template that they strove to copy and whose success they hoped to approximate.

Cole divides the history of Crusade preaching into two phases. The twelfth century was the period of personal preaching, when persons charged with raising Crusading armies selected their own itineraries, made up their own timetables, found their own interpreters and other assistants, and in general followed the course dictated by their own experience, contacts, and whatever inspiration they could muster. This way of doing things changed fundamentally about the beginning of the thirteenth century. The changes are linked to the reconceptualization of the Crusades that began with the pontificate of Innocent III. Crusades hereafter became increasingly institutionalized. Successive popes encouraged preachers to recruit volunteers who would redeem their Crusading vows for money rather than discharge them by

personal participation in an expedition. Changes in preaching practice inevitably followed.

Cole's book marks an important first step in the investigation and analysis of this process, but it is only a beginning. One is entitled to hope that she will pursue further some of the avenues she has marked out here and that other scholars may take her work as a point of departure for additional investigations. Important desiderata would include the identification and publication of further Crusade sermons and an edition of the treatise on Crusade preaching by Humbert of Romans.

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GEORGE W. DAMERON. *Episcopal Power and Florentine Society, 1000–1320*. (Harvard Historical Studies, number 107.) Cambridge: Harvard University Press. 1991. Pp. xi, 284. \$45.00.

This excellent book by George W. Dameron surveys the economic development of the Florentine episcopacy from 1000 to 1320. The internal dynamics of the bishopric is divided into three phases: 1000–1180, 1180–1250, and 1250–1320. The author portrays the bishops not as anachronistic, "feudal" landlords but rather as enterprising administrators who responded creatively to pressures placed on them by aristocratic families, rural communes, nearby towns, margraves, urban guilds, popes, cathedral chapters, and in general by the economic changes occurring in Tuscany. Having no long-range economic goals, the bishops reacted effectively to the growth in commercial markets, industrial and agricultural production, inflation, population increase, and the commune's taxes. Dameron downplays the impact of the Gregorian reforms on the diocese, preferring instead to set the bishop and his allies against anti-episcopal factions.

The best parts of the book deal with the period 1000–1200, when the bishops—often allied haphazardly with the commune of Florence—expanded their jurisdiction in the *contado*. The author synthesizes many previous studies while adding his own interpretations of episcopal registers. Dameron describes the episcopal competition with rural communes and the patrilineages over holdings throughout the diocese. Lacking comital status, the bishops had to contend for territorial rights in the river valleys.

The author's treatment of 1250–1320 seems less focused. He stresses the bishops' ingenuity in preserving their income even as their jurisdiction waned, the magnates' use of ecclesiastical connections to compensate for their loss of political power after 1293, the resistance of rural communes to episcopal initiatives, and the symbiotic economic relationship between commune and bishopric. He argues persuasively that

the bishops passed from territorial lordship to "land lordship" (control over a smaller but more consolidated and efficiently managed estate).

Dameron perhaps overcorrects the views of earlier scholars, such as Robert Davidsohn and Gioacchino Volpe, in underestimating spiritual motives for church reform. Driven by ambition, Dameron's bishops cynically exploited their spiritual resources for personal gain. They concocted saints' cults to outwit their opponents. All episcopal actions are "strategies" and power plays. Even Abbott Gualberto, a protégé of Romuald, was moved to assail Alberga out of "patriarchy and hostility to women" (p. 31). Indeed, Gualberto's supporters opposed simony, according to Dameron, because they feared the spread of a monetized market economy (pp. 31, 42). The religious dimension of the investiture controversy is lost on the author, who reduces all conflicts between bishops and others to economic rivalry. Dameron warns the reader not to apply later standards to eleventh-century Tuscany (p. 188); he does not always follow his own advice.

Dameron concentrates on the uniqueness of Florence's bishopric. He makes comparisons with other Tuscan towns and dioceses to illustrate its characteristics. Hence, the wider significance of Florence's development is not always apparent. But within the confines of his objectives the author accomplishes his task admirably. The Florentine bishopric has been too little studied. This book is highly recommended.

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MARIE-CHRISTINE POUCHELLE. *The Body and Surgery in the Middle Ages*. Translated by ROSEMARY MORRIS. New Brunswick, N.J.: Rutgers University Press. 1990. Pp. vi, 276. \$40.00.

At its core, Marie-Christine Pouchelle's book is an examination of the surgical text of Henri de Mondeville (ca. 1260–1320), an innovative French medical practitioner who taught at the universities at Montpellier and Paris and rose to be royal surgeon. Mondeville conceived his work as a *summa*, a synthesis of both the practical and theoretical knowledge available at the time. But Pouchelle aims to write more than a history of medico-surgical learning. She tries to put herself in Mondeville's place, seeing what he saw, looking at his subconscious processes, and even studying his dreams. In short, this is not a standard history, although the first part of the book deals admirably with the more traditional aspects of medical history.

It is in the second part of the book, where Pouchelle turns to a study of the body and its diseases, that she gives free reign to her imagination. Mondeville initially had classified and analyzed areas of the body, and then had compared the body to other things, animate and inanimate. Pouchelle in

following this path proceeds to give an "anthropological structure of the imagination" (p. 96). This part of the book is a tour de force, an attempt to integrate folk belief, metaphor, and semantics into a better understanding of the medieval ideas about the body. She argues that her methodology is justified because Mondeville was attempting to establish a new architecture of the body and this involved not only an understanding of anatomy and physiology but also of the body politic, that is, society as a body. To do this she ranges widely over medieval concepts on a variety of subjects from prisons and fortresses to ships. Almost everything becomes grist for her mill.

Particularly interesting, and particularly original, are the similes she draws between the sex organs of males and females and everyday life. Men were noble because their genitals are exposed and open to view and "nobly" erect. Women are mysterious and secretive because theirs are not. Thus, men were meant to rule and to travel widely while women were to remain at home; "nesting," so to speak. She elaborates these and other ideas in a discussion of the architecture of the body. Equally interesting is her analysis of the body under attack from nature in which Mondeville drew metaphors from the natural world to illustrate his concepts. In the process Pouchelle turns to examples from modern folk healers to show similarities. She argues that in order to understand medieval medicine, it is necessary to bring together ethnography, psychoanalysis, and biology.

The result is a passionately involved book that is not in the tradition of objective history. Rather it is a personal, highly impressionistic account, based on exhaustive scholarship and analysis, and is both stimulating and provocative. There are many points where I think she has overstated her case, but the result still is a welcome addition to medieval scholarship and should prove useful to students and teachers alike.

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CAROLYN POLING SCHRIBER. *The Dilemma of Arnulf of Lisieux: New Ideas versus Old Ideals*. Bloomington: Indiana University Press. 1990. Pp. xx, 183. \$27.50.

Arnulf, bishop of Lisieux from 1141 to 1181, played a prominent role in and beyond the Anglo-Norman realm: polemicist for Innocent II and Alexander III in the disputed papal elections of 1130 and 1159; defender of King Stephen's accession at the papal court in 1139; papal legate to the Anglo-Norman troops on the Second Crusade; envoy of France to the Angevin court in 1149; supporter of Henry of Anjou's claim to the Anglo-Norman dominions in the early 1150s; justiciar of Normandy in the 1150s; preacher of a sermon at the Council of Tours in

1163; royal diplomat and would-be peacemaker during the Becket controversy; author of a noted collection of letters; and mastermind of the building of that early Gothic monument, the cathedral of Lisieux. His activities brought him not only prominence and wealth but also trouble and grief: quarrels with his cathedral clergy; scorn as a time-server from ardent spirits in the disputes of his day; a loss of royal favor so great that he was forced from his bishopric; and an enduring reputation for dishonesty.

Carolyn Poling Schriber sets Arnulf's life in a context that serves to rehabilitate his reputation. She argues that Arnulf's accomplishments and difficulties were caused by his adherence to ideals acquired in his early years. His "paradigm" (a word that occurs often in this work) was a model of the ideal bishop: combining magnificence with sanctity, recognizing the need of church and state for each other's support, and being able to arrange successful compromises between *regnum* and *sacerdotium*—which determined his decisions when confronted with the "difficult choices" and "conflicting loyalties" (p. xix) of the twelfth century. The paradigm even explains elements of the design of Lisieux cathedral, but it grew increasingly out of touch with reality in Arnulf's later years.

Schriber consciously bases her thesis on Thomas Kuhn's explanation of scientific revolutions. Sympathetic and intriguing, her thesis rescues Arnulf from his reputation for double-dealing and restores him to the world of individuals who act from motives that seem creditable to them. While plausible, however, it is not entirely persuasive. Too little is known about Arnulf's early years to make a detailed description of his intellectual development possible, and not enough attention is devoted to ideas in Arnulf's writings that might substantiate the claims made about his paradigm. Nor are the differences between Arnulf's paradigm and the one with which it eventually came into conflict adequately defined, especially since an almost exactly opposite portrait could be and has been given of the generations of the twelfth century, one in which the pragmatists follow rather than precede the doctrinaires. The book could also have used more assiduous copyediting and proofreading.

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PATRICK SIMS-WILLIAMS. *Religion and Literature in Western England, 600–800*. (Cambridge Studies in Anglo-Saxon England, number 3.) New York: Cambridge University Press. 1990. Pp. xiii, 448. \$65.00.

The most astonishing thing about this superb monograph is that it virtually creates a subject that, given its dating framework, might have seemed not to exist. The first two thirds of Patrick Sims-Williams's period, ca. 600–730, is exactly the span of years treated so vividly by Bede in his *Ecclesiastical History of the English*

People; because Bede had relatively little to say about the west of England in comparison with his full treatment of the southeast and his native Northumbria, the west has hitherto not seemed worthy of much attention. By contrast, the period from ca. 730 to 800 is dominated by two Mercian kings, Aethelbald and Offa, about whose reigns the historians' commonplace is that, in the absence of Mercian-based narrative sources, we can never have a full picture. If this has been accepted as true of the main Mercian kingdom, then it seems to be even more the case for the two sub-kingdoms Sims-Williams concentrates on, those of the somewhat shadowy peoples called the Hwicce and the Magonsaetan.

Despite these difficulties, Sims-Williams has opened up period and region alike through concentration on the church. The dioceses of Worcester and Hereford provide both a firmer structure for the area than the often-shifting political arrangements and the setting for a quite amazing cultural flowering, one which might have been mentioned in the same breath as the "Northumbrian Renaissance" had we but known about it—as we now do.

Monasticism is the most important, or at least most palpable, expression of church life studied here. The monastic foundations of the west were among the largest landholders in the area and, along with the incipient cathedral establishments at Worcester and Hereford, the centers of high literary productivity. The variety of this productivity is quite amazing, ranging from the apocalyptic vision of an anonymous monk of Wenlock to quasi-devotional charms to a considerable body of ambitious Anglo-Latin poetry written as well as collected by Milred, the mid-eight-century bishop of Worcester.

Indeed, the riches of the sections dealing with this literature elicit the only serious cavil to be made: that the book's title is inadequate. Few readers would suspect that a book of this title would include, *inter alia*, probably the most detailed treatment we have of letter-writing in Anglo-Saxon England, a minute dissection of the four surviving early English prayer books, or an equally meticulous handling of epigraphy in Latin verse, summed up by the nicely rounded statement, "Epigraphy integrated the Anglo-Saxon church, for all the uncouthness of its personal names, into the Universal Church" (p. 348).

A more informative title would certainly have included the word "culture"—an aspect in fact central to this work; the centrality of culture would also have been emphasized more overtly had at least a few illustrations been provided, especially of some of the many manuscripts discussed. To be balanced against this lack are the translations thoughtfully supplied for all the numerous bits of Latin quoted, much of it extremely difficult, and the lengthy bibliography and full index that conclude the book. Overall, this is a masterpiece, worthy to become a permanent shelf-

mate to Wilhelm Levison's classic *England and the Continent in the Eighth Century* (1946).

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GERVASE ROSSER, *Medieval Westminster, 1200–1540*. New York: Clarendon Press of Oxford University Press. 1989. Pp. xvii, 425. \$74.00.

Gervase Rosser has followed his several articles on late-medieval towns and vills with a book on medieval Westminster. This detailed case study depends primarily on documentation from Westminster Abbey, the vill's largest property holder, supplemented by guild and churchwardens' records, wills, literary remains, and royal archives. The focus is only peripherally on the royal court. Rosser concentrates instead on the town itself: its physical layout; its occupations, population shifts, and social structure; its communal strengths and forms of governance; and, finally, its religious, charitable, and cultural life. In the course of this survey, Rosser encounters many of the ongoing debates regarding late-medieval and Reformation England.

Westminster was unique—a suburb of London that housed the royal court as well as one of England's wealthiest abbeys. It was a pilgrimage site, a sanctuary, and a commercial center. Due to its loose form of government, "at a stage between that of a manorial village and that of an incorporated town" (p. 282), and its privileged status vis-à-vis London, it attracted more than its share of transients (from a wide catchment area) and a substantial amount of wealth.

Basing his argument on rental returns, land market patterns, and rough population estimates from 1407–08, 1524, and the 1540s, Rosser postulates a thriving community up to about 1300, and thereafter a probable but not provable decline; economic recovery from about 1370 to 1410, a general economic and population slump for the succeeding sixty years; and a notable demographic recovery after 1470. This last recovery did not necessarily translate into economic health, however, as it brought increasing numbers of indigents into Westminster. As a contribution, therefore, to the larger debate on urban decline in the late-medieval and sixteenth centuries, the evidence from Westminster is ambiguous: "There is no simple sense in which Westminster either 'declined' or 'prospered' in the late middle ages, and in general, such blanket terms are best avoided by historians" (p. 325). Rosser's study underlines the value of a case-by-case assessment.

Rosser describes a late-medieval society that was becoming laicized. The abbey's control of properties was more and more indirect, through lay landlords; royal offices became increasingly accessible to educated laity; and lay townspeople governed, through guild and manor court, arenas that had once been

ruled by the abbey's officials. Late-medieval Westminster was also a religiously committed and activist society, particularly at the parish level. Despite a monastic community that was no longer central to lay piety and the scandalous behavior of a few ecclesiastical brethren, there was no obvious anticlericalism or religious lassitude. Rosser's study supports neither the idea of an imposed or a popular Reformation but rather a middle ground of continuity and religious creativity that transcended the Reformation.

Rosser lends much support to the view that community in late-medieval England did evolve and can be examined independent of legal definitions or the existing judicial framework. There is a refreshing effort on his part to integrate women throughout the text, despite a small, ghettoizing section entitled "Women." The book has other small flaws as well. One wishes for more evidence to fill the gaps—prior to 1250 and particularly from 1300 to 1370; Rosser's assumption that Chaucer's audience was the townspeople of Westminster and London is arguable; and the inadequate index can be frustrating.

Rosser enlivens his study with anecdotes and with accounts of drinking houses, prostitution, and poverty. Occasionally flip but generally fair, this book is a measured, markedly good contribution to a field that needs more specific case studies such as this and fewer generalities.

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W. M. ORMROD. *The Reign of Edward III: Crown and Political Society in England, 1327–1377*. New Haven: Yale University Press. 1990. Pp. xiii, 280. \$30.00.

Recent scholarship on later medieval England has been marked by a new emphasis on King Edward III. W. M. Ormrod's book is an important addition to those of G. L. Harriss, Juliet Vale, Chris Given-Wilson, Scott Waugh, and others in providing a fresh examination and appraisal of Edward's reign and era. Collectively these writers have successfully challenged the commonplace image of Edward as a one-dimensional, superficial warrior-king. Ormrod explicitly and directly takes issue with that image; his particular objective is to refute the proposition that Edward carelessly, even recklessly, sacrificed or compromised royal authority for the sake of his war agenda. The book is vigorously written and up to date in incorporating significant research. Besides the scholars just mentioned, Ormrod has made good use, for example, of the work of Nigel Saul and J. M. W. Bean on the gentry and on "bastard feudalism." He has, of course, incorporated and built on his own spate of recent major articles as well.

The thesis of the book is that Edward had a decisive, consistent, and effective domestic policy, one that strengthened rather than weakened the monar-

chy by coopting and mastering political society, both within and outside of Parliament. After a brief narrative overview, Ormrod devotes the bulk of his study to a series of analytical chapters focused on governance and administration and on relations, both personal and institutional, between king and leading minister-advisers, the higher nobility, the clergy, provincial gentry, and urban elites. He readily admits the problems and mistakes of the 1330s, even labeling the assumption of the French royal title a "publicity stunt" (p. 10). But he shows persuasively that the way the king faced and surmounted the crisis of 1340–41 led squarely to a consequent full quarter-century of personal popularity, political stability, and the effective restoration and implementation of royal authority. The image of the glamorous and victorious warrior counterbalanced the realities of decline in the 1370s; Ormrod points out that this image has obscured Edward's very real domestic objectives and accomplishments as well. The overall argument is important, well-constructed, and convincing.

A number of English monarchs are now being reassessed in more favorable or sympathetic terms, including Henry III (by David A. Carpenter) and Mary Tudor (by David M. Loades). The Edward III who emerges from Ormrod's pages is another example of this pattern. Ormrod has not attempted to write a large-scale biography (any effort to do so would have had to include a direct study of the Scots and French wars, in any event), but rather a political profile of royal style and policy. His achievement will help decisively to shape and inform any future serious biography.

MICHAEL ALTSCHUL
Case Western Reserve University

NORMAN DOE. *Fundamental Authority in Late Medieval English Law*. (Cambridge Studies in English Legal History.) New York: Cambridge University Press. 1990. Pp. xxi, 197.

This legal study by Norman Doe is a remarkably good book not because it is the first to deal with the foundations and principles of late medieval law but because it is the first to explain with exceptional clarity in what manner English legal theorists and practitioners of law viewed such matters as authority, consent, human law, natural law, justice, conscience, mischief, and inconvenience. Even more remarkable is the comparative perspective of the author. Whereas most English legal historians have hewed to an insular view of the common law, continuing on in the tradition of Sir Edward Coke, Doe constantly compares English law with Continental law and underlines that what was thought and done about the law in England was not unlike what was thought about it and done about it on the Continent. He shows that the ideas of Reginald Pecock, John Fortescue, Thomas Littleton, and Saint Germain on divine, natural, and human

law, justice, reason, equity, authority, and consent to legislation were in harmony with the ideas of such notables as Bartolus of Sassoferrato, Thomas Aquinas, Marsilio of Padua, Pierre D'Ailly, Jean Gerson, and Nicholas of Cusa.

There is much to learn from this book. Until the fifteenth century it was the writ system that was "the focal point of legal practice and study" (p. 1). Only in the fifteenth century did substantive law develop and begin to replace the *Register of Writs* as the font of what English lawyers looked to for guidance. During the fifteenth century law came to be viewed from two levels—the theoretical and the practical. Fortescue might well theorize this and that about reason, justice, equity, and conscience in his *De Laudibus Legum Angliae*, but in the law courts the judges and lawyers took a practical view of such concepts as they impinging on cases involving deceit, contract, detainee, and gavelkind. There was disagreement and tension between the theoretical and the practical but accommodations were made. For example, in cases and opinions cited by Doe, words such as reason, justice, and conscience are commonly used by judge and lawyer. In his informative chapter on "Conscience in the Common Law," Doe overturns the traditional view that conscience and equity were the singular preserve of the medieval chancellor and his courts. He demonstrates that "Time and time again practitioners, the legislators and the judges, employed conscience as the basis for their decisions" (p. 136).

On the practical concerns of lawyers and judges, Doe has much to say. For example, when the lawyer referred to inconvenience he did so to exclude inconsistent results. The lawyers used inconvenience on two levels: "to prohibit a judicial decision from being inconsistent with established practice and decisions in previous cases; and to prohibit decisions made in one case which are inconsistent with other decisions made in the same case" (pp. 162–63). Above all, the fifteenth-century lawyer used inconvenience as well as reason to achieve the objective of legal symmetry and good sense.

In his concluding chapter Doe states that the evidence examined enables him to assert that in the fifteenth century there is the beginning of a positivist idea of law, that law did not have to be divine, that it could be of human origin, and that human law could be divorced from abstract right and wrong. This fine book should be compulsory reading for both legal historian and political theorist.

BRYCE LYON
Brown University

IU. G. ALEKSEEV. *Osvobozhdenie Rusi ot ordynskogo iga* [The Liberation of Rus' from the Yoke of the Golden Horde]. Leningrad: Nauka. 1989. Pp. 217. 1 r. 50 k.

This study of relations between Russia and the Golden Horde from the 1460s leads up to a single

campaign, the stand on the Ugra River of 1480, which has been studied by a series of Soviet scholars including K. V. Bazilevich (*Vneshniaia politika Russkogo tsentralizovannogo gosudarstva. Vtoraia polovina XV veka* [1952]) and more recently V. V. Kargalov, Ia. S. Lur'e, B. D. Nazarov, and D. N. Shanskii. Iu. G. Alekseev also examines issues in Russian history, going far beyond what the title of this work suggests, to offer a context for this event. For this reason his work can be regarded as the chronological continuation of that of L. V. Cherepnin (*Obrazovanie Russkogo tsentralizovannogo gosudarstva v XIV–XV vekakh. Ocherki sotsial'no-ekonomicheskoi i politicheskoi istorii Rusi* [1960]).

The author places Muscovy's relations with the Golden Horde within the context of the emergence of a centralized Russian state from the 1460s–70s. Although this process results in increased opposition to Muscovite domination by the appanage dukes and the clerics, Alekseev considers it to have been an important factor in the Russian victory of 1480. The main focus of the book, however, is on the relations between Muscovy and the Horde, which also entails the complicated alliances of the Horde with Lithuania and Muscovy with the Crimea.

Alekseev claims that Akhmat Khan of the Great Horde attempted to reestablish the earlier might of the Chinggisids on an international scale beginning with his campaign against Moscow in 1472. The author attributes the Russian victory to the rapid deployment of forces, which now included troops of various dukes subject to Moscow as well as Tatar troops in Russian service on the Oka River in defense of Moscow. The author argues that Ivan III was the first Grand Prince to assume his position without the sanction of the Horde ruler, but that tribute relations continued until the withdrawal of Akhmat's forces in 1472. The next major confrontation between Russia and the Horde in 1480 Alekseev portrays as an attempt to restore the payment of tribute and as central to the fate of Russia. Rather than engaging in a decisive battle, however, Akhmat withdrew without taking Moscow.

The author of this work makes the same assumptions as others steeped in the traditions of the nationalist school of historiography of medieval Russia. One of these assumptions, reflected in the title of this work, is that the stand on the Ugra River constituted a watershed event in Russian history, supposedly resulting in the liberation of Russia from the "Tatar Yoke." Indeed, the author is more concerned with proving this a priori assumption than in investigating the events of the period from any alternative perspective. Historians should be disappointed by Alekseev's dismissal of the outbreak of an epidemic as a factor in the early withdrawal of Akhmat's forces on August 1, 1472. Alekseev does mention details regarding the use of improved artillery during the stand on the Ugra River in 1480, but he does not elaborate on its potential significance. He also does not consider the late date in the year as a factor which might have

compelled Akhmat to withdraw his forces to winter quarters on November 11, 1480, to ensure their survival.

Moreover, Alekseev considers Russian-language primary sources and the Soviet secondary literature for medieval Russia sufficient for a study including the Golden Horde, which is portrayed as a state with no internal history of its own. The author ignores the substantial Soviet literature on Muscovy's neighbors to the east and Western scholarship on all topics, even where it would have been important for a reevaluation of the primary sources (the works of E. L. Keenan, Jr., and C. Halperin come to mind).

Alekseev's most important consideration, evidently, is to claim a Russian victory so that November 12, 1480 can be called the first full day of a free Russian state (p. 116). This leaves unaddressed important questions such as whether the Horde of Akhmat Khan really represented a continuation of the Chinggisid state of the thirteenth–fourteenth centuries and what Muscovy's position was vis-à-vis the khانات of Kazan and the Crimea after 1480. The author's enthusiasm about the Russian victory of 1480 is called into question even more by another event described, the destruction of Akhmat's army and the murder of Akhmat Khan himself in 1481 by Shaybanid forces. This was the real defeat for Akhmat Khan.

Alekseev's study would be more interesting had he proceeded from his own comment that the stand on the Ugra River was not considered a decisive victory at the time (p. 133) and examined why it came to be considered important only later.

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MODERN EUROPE

ROBERT BIRELEY. *The Counter-Reformation Prince: Anti-Machiavellianism or Catholic Statecraft in Early Modern Europe*. Chapel Hill: University of North Carolina Press. 1990. Pp. xii, 309. \$39.95.

Six anti-Machiavellians—Giovanni Botero, Justus Lipsius, Pedro de Ribadeneira, Diego Saavedra Fajardo, Adam Contzen, and Carlo Scribani—all either Jesuits or part of the Counter Reformation, are the subject of Robert Bireley's study. Each receives a chapter of analysis with comparisons to the others and to the late scholastics. They are called anti-Machiavellian because they so identified themselves and because they attempted to reconcile Christian doctrine with success in politics, contrary to Machiavelli's view that a sincere Christian "learns his ruin" (p. 1). Thus, to do justice to his subjects, Bireley is compelled to accept the understanding of Machiavelli as "Machiavellian" or evil, since otherwise it would have been senseless for them to refute him. Bireley

accepts this premise with qualms, resulting from unwillingness to challenge modern Machiavelli scholarship (which does not believe that Machiavelli was Machiavellian), but he does take the minority view that Machiavelli was an atheist.

In the case of the anti-Machiavellians, however, Bireley brings out that each "apparently embrac[ed] elements of Machiavellianism" (p. 92). In each case the concession goes beyond "the desire to meet Machiavelli on his own terms" (p. 238), since they adopted so many of those terms and did their best to promote state power. Machiavelli himself raised the possibility of interpreting Christianity "according to virtue"—his own virtue—rather than rejecting it (*Discourses on Livy*, II, 2). And it is easy to believe that Machiavelli would have forgiven in a follower the fraud of pretending to be against him while actually furthering his enterprise and of feigning to be Christian while actually perverting Christianity. What prevents the anti-Machiavellians from being hidden, or partially hidden, Machiavellians, acting in the new circumstances of religious conflict among Christians? In their time the danger was not Christian softness but Christian zeal, and the remedy was toleration rather than manipulation. But was their worldliness not in sum closer to Machiavelli than to Loyola?

Bireley has perhaps considered that possibility, but he does not face it squarely. In order to set down the issues between Machiavelli and the anti-Machiavellians, he rightly begins with a summary of Machiavelli. He does not, however, discuss Machiavelli's novelty and his ambition. Nor does he refer to the argument of Leo Strauss's *Thoughts on Machiavelli* (1958) that deliberate fraud was necessary to Machiavelli's ambition. Furthermore, he does not adequately consider Tacitism (furthering Machiavelli's influence under the name of Tacitus) and the resemblance of his anti-Machiavellians to the *politiques*, also commonly accepted examples of intellectual frauds serving Machiavellian goals. Bireley seems to believe the words he puts in the mouth of Botero: one "could not successfully counterfeit piety over a long period of time" (p. 55). But if professions of piety are accepted at face value, as they tend to be taken by scholars today, success in counterfeiting is almost guaranteed. The result is a very great distortion of history, obscuring the origins of secular modernity.

Bireley gives able and judicious accounts of his anti-Machiavellians, but in failing to challenge the conventional presumption he cooperates in the distortion. His analysis of the texts is too trusting and too easily diverted by the writers' devices of self-preservation. Bireley is not sufficiently searching, penetrating—or inquisitorial. What he needs is the sharp suspicion, without the moral indignation, of an Inquisitor.

HARVEY C. MANSFIELD, JR.
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JOHANNES MENNE POSTMA. *The Dutch in the Atlantic Slave Trade, 1600–1815*. New York: Cambridge University Press. 1990. Pp. xiv, 428. \$54.50.

During the past few decades, extensive archival research and imaginative quantification have reinvigorated the study of the transatlantic slave trade. In this wide-ranging and informative book, based on information culled from numerous archives in the Netherlands and presented in dozens of tables, figures, and appendixes, Johannes Menne Postma examines Dutch participation in this mournful business. Combining chronological and topical approaches, Postma interweaves a narrative of the rise and fall of a system that extended over three continents and more than two centuries with an analysis of the system's structures and operation.

Initially conducted by individual skippers, Dutch slaving was monopolized by the West India Company between the 1630s and the 1730s. The trade focused first on northern Brazil during a brief period of Netherlands rule (1630–54), and thereafter on the *asiento* trade to the Spanish colonies. The trade reached its height supplying slaves to plantations in Surinam and the neighboring Guiana colonies in the four decades after the West Indies Company lost its monopoly. Beginning in 1773, however, international economic crisis and the conflicts spawned by the American and French revolutions provoked swift and irreversible decline.

Postma investigates the Dutch roots of slaving and the institutions involved. But he devotes most attention to Africa and the Caribbean, carefully delineating the acquisition, treatment, and commercialization of the 550,000 West Africans whom, he calculates, Dutch merchants sent to the New World. Much of what he describes is congruent with findings reported in the current literature, although it turns out that Dutch slavers adopted free trade later than their European rivals, earned lower average rates of profit, and inflicted higher mortality rates on their captive passengers. In contrast to some recent historians, Postma argues that the Dutch, at least, were engaged in a true triangular trade. Yet the Dutch experience may represent a variant on a common pattern rather than an exception to it. As Postma acknowledges, many boats sailed directly between Holland and the West Indies, and the majority of slave ships for which firm information exists left the Western Hemisphere for Europe in ballast or with minimal cargoes.

Postma notes with regret that his sources reveal little about the humans caught up in this infernal commerce. The role of the slave trade in stimulating capital accumulation and the Dutch economy in general is also alluded to rather than explored in depth. Still, in addition to its careful and perceptive substantive findings, this book provides a rich lode of documentation. Particularly valuable are the twenty-six appendixes. These hundred pages contain not only

transcriptions of numerous contemporary documents (including the original 1667 *asiento* contract, guidelines for the conduct of the slave trade in West Africa by a factor who spent years there, and slave death certificates) but also a mass of information about 1,210 Dutch slaving voyages between 1675 and 1802.

ROBERT S. DUPLESSIS
Swarthmore College

CONSTANCE JORDAN. *Renaissance Feminism: Literary Texts and Political Models*. Ithaca: Cornell University Press. 1990. Pp. x, 319. Cloth \$42.50, paper \$12.95.

Constance Jordan's choice of the word "feminism" for her title will certainly provoke criticism from those who have not read the book. I doubt, however, that anyone could finish this finely argued discussion without agreeing that the pro-woman side of the Renaissance *querelle des femmes* included many ideas that are key parts of contemporary feminist analysis. Jordan discusses the ideas of writers from Giovanni Boccaccio to the anonymous authors of the pamphlets *Hic Mulier* and *Haec Vir* (1620), finding in many of these ideas a clear recognition of the distinction between power and authority, the link between women's secondary status and restrictions on their control of property, the cultural roots of a hierarchical valuation of men's and women's work, the interplay between distinctions of gender and those of rank, and the gendered nature of language.

Although Jordan is a member of an English department, her book is much more intellectual history than literary criticism, for she traces lines of argument rather than surveys the final opinions of individuals or analyzes texts as a whole. The book is therefore arranged by topic rather than author and includes only works that were printed in some vernacular, for only these could be taken up by the range of authors whom Jordan investigates. By writing this as intellectual history, Jordan is not limited by genre and includes extensive discussion of Philip Sidney's *Arcadia* and François Rabelais's *Pantagruel* and *Tiers Livre*, as well as a huge number of works of nonfiction—marriage manuals, homilies, political pamphlets, popular satires, and pieces written specifically in favor of or criticizing women. She is also able to trace how feminist and antifeminist arguments changed and developed over the nearly 250 years she surveys and make conclusions as to national differences, for each chapter includes separate sections on Italy, France, and England.

While most of the works that Jordan analyzes focus on women, marriage, or the family, so that ideas about gender must be extrapolated from them, a surprising number explicitly discuss what we would now phrase the social construction of gender. Works on queenship include discussions of androgyny; pro-female treatises point out the difference between "feminine" and "effeminate" and stress the "femi-

nine" side of such qualities as equity; political treatises recognize the links between the power of the husband and that of the monarch and the gendered nature of political subjection. These insights would remain theoretical throughout the period, however, for, as Jordan both notes and criticizes in her authors, none moved beyond analysis to concrete demands for change. Some of those who expressed quite radical ideas in one place retreated to more conservative opinions elsewhere, or remained ambiguous; they advocated companionate marriage yet justified wife beating, or saw the possibility of equality between spouses yet described the ideal marriage as a hierarchical one.

This is one of the few books that really should be read by all the groups suggested on the jacket cover. Jordan's strict avoidance of the postmodernist jargon that has made so much recent Renaissance scholarship impenetrable to the uninitiated opens this work to anyone simply willing to read carefully, although I would suggest reading the notes concurrently, for Jordan occasionally buries important points there.

MERRY E. WIESNER
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GARY CROSS. *A Quest for Time: The Reduction of Work in Britain and France, 1840–1940*. Berkeley and Los Angeles: University of California Press. 1989. Pp. xi, 330. \$39.95.

Gary Cross's study examines the century-long process through which the eight-hour day as well as weekend and vacation leisure came to be accepted as normal. His analysis focuses on the politics of labor legislation, especially concerning the establishment of the eight-hour day in 1919. Cross, however, accomplishes significantly more in this stimulating work. He reconsiders the transformation of the British and French working class in the late nineteenth and early twentieth centuries. This in turn prompts reviews of the relations between social classes and the state, the origins and consequences of legislated reforms, the unexamined assumptions of much labor history, and the new insights of gender, family, and cultural studies.

These challenging issues remain rooted in the examination of the campaign for short hours, which Cross identifies as the central demand of organized labor. This campaign challenged the ideology and practices of nineteenth-century liberal politics and economics. Cross's insistence on the intransigent opposition of most British and French employers to any hour reform is a useful reminder of the power of market forces and ideology. The increase in leisure time eventually gained by French and British workers was the result of an interclass political coalition of organized labor, socialists, and bourgeois reformers.

The ability of these coalitions to overcome the powerful resistance of employers and their political allies depended on new attitudes toward leisure and the family, the international militancy of labor, and an international conjuncture (especially between 1917 and 1920) which temporarily muted market pressures. Cross argues persuasively that rank-and-file workers supported short-hour reforms that brought them significant economic and cultural benefits. Cross also indicates aspects of this transformation that may have had less positive consequences, such as linking increased leisure to the privatized family, which evolved at the expense of work-related and political forms of sociability. Cross concludes that new working-class attitudes toward leisure, developed between 1840 and 1940, were central to securing greater free time, providing workers with more personal leisure for family life and individual dignity.

This study makes important contributions, particularly in its attention to the origins and consequences of labor reform and in its argument for the autonomous development of a new working-class leisure and work ethic. But in his laudable effort to cover long-term developments within a comparative context, Cross perhaps moves too quickly over some complex issues. His identification of the centrality of gender and family in the short-hours debate significantly adds to our understanding of labor reform and new expectations of leisure. For the most part, however, the reformulation of the working-class leisure ethic is given from the perspective of male workers and reformers.

Cross accepts the standard interpretation of gender-specific legislation, which views the regulation of women's working hours as an interim step toward a shorter day for all workers. He does not address recent scholarship such as Mary Lynn Stewart's, who argued that this legislation was intended neither to increase women's leisure nor to promote universal regulation (*Women, Work and the French State: Labour Protection and Social Patriarchy, 1879–1919* [1989]). In addition, Cross does not make clear how, or if, men and women responded differently to the new concepts of leisure that stressed more family time. Associating free time with the family was new for men, but not for women workers. The family might be a leisure arena for men; for women it was primarily a location of work.

The controversies and debates raised by Cross's analysis demonstrate its importance. His study facilitates and requires the reexamination of several major interpretations of leisure, labor legislation, the restructuring of the working class, the dynamic of reform politics, and the significance of gender and the family. Cross has convincingly developed new directions in the study of the modern state and society under a democratic capitalist system.

JUDITH F. STONE
Western Michigan University

MICHAEL SEIDMAN. *Workers against Work: Labor in Paris and Barcelona during the Popular Fronts*. Berkeley and Los Angeles: University of California Press. 1991. Pp. xiv, 399. \$39.95.

Michael Seidman's book is a provocative and insightful comparative study of workers in Barcelona and Paris in the Popular Front era. He argues that workers' apathy, indifference, and resistance to work were as, if not more, important than revolutionary aspirations, class antagonisms, and ideological combat in shaping the outcomes of their struggles. Revolutionary Catalonia, with Barcelona its central arena of experiment, demonstrates the tenacity of ideologies, the effects of a bourgeoisie's reluctance to embrace dynamic industrialization, and the radicalizing power of prolonged experience of poverty and misery. France, on the contrary, is viewed as an advanced industrial nation whose working class, as represented in the Paris region, was little interested in revolutionary change and zealous only in its claims for more pay and time off. Despite these clear differences, Seidman believes that workers in both countries responded to demands for more work with similar stubborn resistance. Thus, their actions in some distinct measure undermined the goals that their leaders proclaimed, chief among them the need to increase production and enhance the industrial power of their countries.

The argument is most fascinating and persuasive when applied to Barcelona's workers. Seidman's interpretation of the evidence suggests that anarcho-syndicalism was a rational response to social and economic conditions, not a millenarian fantasy. Anarchist leaders thought that salvation was to be found in work, indeed that the revolutionary task was to advance Catalonia and Spain to the economic level of more advanced countries. Workers thought and acted otherwise. In the summer of 1936, anarchist militants seized the means of production, aiming to install rational organization and to inspire, sometimes to coerce, labor to produce more, all the while calling for the heroic sacrifice needed in wartime. Workers responded, in a diffuse and unarticulated way, with absenteeism, fake illnesses, alcoholism, theft, and sabotage, all a "daily negation" (p. 169) of the revolution's values. Anarchist militants were seen less as liberators than as new bosses making ever-increasing coercive demands.

In examining Parisian workers during the Popular Front, Seidman applies his insights with an insistence that minimizes or modifies certain traditional interpretations. For example, French economic development is rarely seen as daring, consistent, or innovative. Yet Seidman argues that "France had steadily and consistently industrialized from the middle of the nineteenth century and throughout the twentieth" (p. 173). Resistance by much of the bourgeoisie to modern industrial capitalism and the persistently sluggish character of the French economy weakens Seidman's case for the strength and dynamism of the Parisian

bourgeoisie. He seems to believe that his argument, that Parisian workers tenaciously resisted all efforts to increase production, must rest on the existence of a dynamic bourgeoisie. The linkage is dubious and unnecessary.

Seidman also minimizes the ideological tensions in Popular Front France and the genuine appeal of democratic and left-wing political activism. The suggestion that the French Popular Front's distinguishing features were the weekend and mass tourism may be consistent with Seidman's interpretation of workers' attitudes toward work, but it misses the point of the entire experience. Political ideas, appeals, and action were contagious in the France of the 1930s, and workers were not immune to them, whatever their attitudes about work. That workers were non-revolutionary in Popular Front France is clear; so were their parties, unions, and their government, but none of them could escape the ideological fevers and commitments of the moment.

NATHANAEL GREENE
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PETER BALDWIN. *The Politics of Social Solidarity: Class Bases of the European Welfare State, 1875-1975*. New York: Cambridge University Press. 1990. Pp. xiii, 353. \$44.50.

Truth in advertising should have precluded the use of misleading dates in the subtitle of Peter Baldwin's bright and ambitious account of welfare reforms in Europe after World War II. Falsely alerted, the reader begins with inappropriate expectations about a sweeping and consecutive historical rendition. But the author's actual intentions are otherwise. Because he aims to treat no less than five separate countries—Denmark, Sweden, France, Germany, and Great Britain—an exhaustive narrative of each over a full century is unfeasible. Accordingly, Baldwin adopts a typological approach that enables him to move deftly from one example to another. He is like a skillful juggler, forever attempting to keep several balls in the air at once while he handles the rest. It is a fiendishly difficult act to sustain, and the wonder is that he manages so well.

The book contains many laudable virtues and one lamentable defect. First the virtues. They begin with the extraordinarily broad deployment of archival sources and secondary literature. Even if we count Denmark as an analytic foil for the more significant case of Sweden, Baldwin's virtuosity as a researcher is evident in his ability to penetrate four major political traditions and to derive a comprehensible pattern from them. According to his scenario, Otto von Bismarck's Germany took the lead in designing a system of workers' insurance, whereas late-nineteenth-century Sweden developed the first flat-rate model that sought to guarantee equality of benefits despite inequities of social status or fiscal contribu-

tion. Between these poles Britain and France at first adopted social insurance more slowly and uncertainly. Then came a shifting of fronts after 1945 when Anglo-Scandinavian legislation approached a "triumph of egalitarian universalism" (p. 135), whereas France and Germany both failed to do so because they attempted to aid the poor at the expense of the rich and thus incurred stiff bourgeois resistance. By the 1970s, that second phase was superseded, in turn, by a third in which Britain and Sweden moved "back to Bismarck" (that is, they retreated from a flat-rate approach toward an earnings-oriented arrangement), while France and Germany fashioned welfare legislation that represented "the fulfillment . . . of the solidaristic vision" (p. 287).

This notion of solidarity is a key to Baldwin's convincing argument that we cannot grasp his complex story as a morality play: neither as a nefarious attempt by elites to buy off their social inferiors nor as a heroic struggle of the poor and the powerless to achieve social justice. Rather, Baldwin contends, it was everywhere the interplay of solidarist politics, requiring accommodations from both sides, that produced a rich variety of results. Each case presented a unique redistribution of fortunes and risks that cannot be adequately explained solely in terms of crass exploitation or laborist advance. The strength of Baldwin's method and the validity of his judgments confirm his grip on a multiplicity of detail, which others have too often described vaguely or mastered only partially.

The study's deficiency lies in its opening section in which Baldwin examines the pre-1914 background of Sweden and Denmark but largely neglects the other countries. In an "Interlogue" he weakly allows that such thoroughness would be supererogatory. Yet this procedure leaves more than one obvious gap, the most crucial of which is an account of the pioneering precedent of Germany. Baldwin's penchant for classification gets the better of him and he too hastily files the Bismarckian system as "contributory" (p. 21). Here a little more old-fashioned empiricism would have been salutary. The truth is that the early German legislation was not solely financed by employers' and workers' contributions (and certainly that was not Bismarck's own preference). At every level of administration—municipal, regional, and state—the cost of welfare and the necessity of raising new revenues rose massively. Moreover, the definition of "worker" was steadily expanded to include peasants, foresters, domestic servants, and (under separate laws) white-collar employees. Thus, Baldwin's neat typesetting deprives the historical reality of its complexity and dynamism. His assertion that the German model reserved "the blessings of state subsidies for only one group" (p. 88) is therefore obfuscating. Furthermore, this misrepresentation continues to plague the remainder of the analysis because, for lack of a clear target, too many retrospective allusions fall wide of the mark.

Still, the lack of a coherent and sufficiently nuanced description of the German model does not vitiate Baldwin's real contribution, which is to unravel the intricacies of contemporary social security. This he does with dexterity, thereby providing us with a reliable guide to one of the most significant accomplishments of twentieth-century Europe.

ALLAN MITCHELL
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ANNE ORDE. *British Policy and European Reconstruction after the First World War*. New York: Cambridge University Press. 1990. Pp. ix, 357. \$54.50.

Anne Orde's new work provides a companion piece to her *Great Britain and International Security, 1920–1926* (1977). In this new volume, which covers 1918–27 and rests on massive research in Anglo-American archives along with some French and German records and wide reading in the published literature, Orde narrowly hews almost entirely to financial reconstruction: debts, reparations, credits, and East European financial reorganization.

Orde is blessedly clear about what she means by reconstruction. Also among the book's virtues are the chapters on financial revival in Eastern Europe and the passages concerning League of Nations economic conferences at Brussels (1920) and Geneva (1927), topics too often omitted in works on European reconstruction. Most of the book, however, is devoted to the debts/reparations tangle. Here there are few surprises for experts but the compilation of much data. Orde appears to be more at home with debts than with reparations, but refreshingly challenges old assumptions and most of the time does not cast Britain as hero, France as villain, or Germany as helpless victim. Although Orde avoids analysis, comment, and conclusions, she implies that American debt policy was the primary problem. Yet, in rehearsing scheme after proposed scheme, she points out neither that many amounted to dumping the costs of the war on the United States nor that payment schedules in eventual debt agreements with the United States implied greater generosity than is usually assumed.

The chief difficulty in these chapters lies in the writing, which will discourage much of her potential audience. Orde has read a great many documents and recites most of them. To soporific prose, intermittent lack of clarity, and the absence of key factors, which will limit the book to experts who can supply the omissions, one must add lack of selectivity, focus, analysis, and interpretation. A typical passage: "Rathbone then wrote a long letter which he proposed to send to Reading, saying that he was willing to share the burden of French purchases in neutral countries but not to finance the whole of them. He desired close relations with Britain. He told Davis that he would be

willing not to send the letter provided that Chamberlain was given its substance, and he would show it to Reading and Blackett for information" (p. 50). We are not told the significance of Rathbone's views, whether they became policy, or whether the letter was sent. Similarly, in reporting a memo by Jacques Seydoux, Orde notes in passing that he thought "The British believed that they had more to gain from German recovery than from receiving reparations" (p. 170), but she never explains, explores, or comments on this crucial point.

In a conclusion consisting mostly of trade statistics, Orde announces that "Europe had substantially recovered economically by 1927" (p. 323), but she avoids obvious questions about economic trends in the next four years. Moreover, she rightly stresses throughout that the emphasis in the 1920s was on leaving reconstruction to private finance, but she deliberately focuses on government policy. Beyond that, her schema largely divorces governmental economic and financial policies from politics, power considerations, prejudices, fears (of Bolshevism, revolution, and Germany), and the security factors with which the reparations/debt issue was inextricably entwined by both Britain and France. In short, one wonders whether Orde would have been wiser to write one large book examining all facets of the tangle instead of two artificially discrete studies of segments of an admittedly unwieldy problem.

SALLY MARKS

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J. CALVITT CLARKE III. *Russia and Italy against Hitler: The Bolshevik-Fascist Rapprochement of the 1930s*. Foreword by CLIFFORD FOUST. (Contributions to the Study of World History, number 21.) New York: Greenwood. 1991. Pp. xvii, 218. \$45.00.

Narrowly defined, J. Calvitt Clarke III has written a history of Italo-Soviet relations in the first part of the 1930s. More broadly interpreted, he has analyzed Italy's, and to some extent the Soviet Union's, role in European collective security against Germany and Japan during this period. The title of the book is misleading, since Italian, not Soviet, diplomacy is the subject of scrutiny. Clarke is to be congratulated for his extensive use of Italian sources, particularly embassy and consular reports located in the Foreign Ministry Archives in Rome. But there are no references to comparable Soviet primary sources so that the monograph is written almost entirely from the Italian perspective.

Clarke's principal theme is that ideology was ultimately insignificant to Italo-Soviet relations and that there were interesting commonalities between fascism and communism. Both Italy and the Soviet Union sought to create a modern industrial state from an underdeveloped nation. They simultaneously cooperated and competed in southeastern Europe. Ac-

cording to Clarke, divergent ideologies did not cause the breakdown in good relations that had existed between the two countries from the autumn of 1924 to 1935. In the final analysis, Benito Mussolini chose not to align Rome with Moscow for practical reasons. The Italians, the author argues, interpreted the Soviet Five-Year plans as a sign of weakness, not of vitality. Yet until 1935 a symbiotic commercial relationship was chiefly responsible for Italo-Soviet diplomatic, political, and military cooperation.

As Clarke correctly asks, why did this collaboration, so essential to European collective security, fail? In his view, not for lack of effort by the Soviets, who encouraged Italo-French cooperation and opposed German penetration into southeastern Europe. The Soviets even worked to remove any threat to collective security caused by Italian action in Abyssinia. In the end, the author blames Britain for demanding economic sanctions by the League of Nations to punish Italy for invading Abyssinia. Forced to decide between Italy and Britain, the Soviet Union chose Britain, as the stronger power, to use against its chief enemies, Germany and Japan. If this account is correct, the British missed a number of opportunities, since the Soviets generally wanted to put Germany in a "straight jacket of peace" by preferring imperial integrity over European security.

Clarke has produced a well-researched and well-written book. But he clearly focuses on Italy, not on the Soviet Union. Now that he has initiated fruitful discussion of Italo-Soviet relations in the 1930s, other historians ought to fill in the gap by examining these same events from the Soviet point of view.

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ALAN F. WILT. *War from the Top: German and British Military Decision Making during World War II*. Bloomington: Indiana University Press. 1990. Pp. ix, 390. \$35.00.

Eschewing any exotic theoretical discussions of the problems of decision making, Alan F. Wilt opts for a straightforward empirical approach. He discusses the performances of Adolf Hitler and Winston Churchill as warlords, outlines the German and British high command structures, gives a brief account of some of the strategies they worked out, and then describes how they were put into effect in eleven German and British campaigns.

The thumbnail sketches of the leading figures are mostly convincing. Hitler is presented as slightly less of a maniacal amateur than in the widely held version that springs from the self-serving memoirs of the German generals and that was given the magisterial support of Liddell Hart. Similarly, Churchill is not quite the infallible strategic genius of Martin Gilbert's hagiography. The German generals are presented as being almost as nasty as they were in fact, and not as

selfless and apolitical professionals who had the misfortune to serve a lunatic. But to say that Alfred Jodl was a "more admirable person" than Wilhelm Keitel is surely utterly to damn with the very faintest of praise. The end of term reports for the British commanders are perhaps a little too favorable. Bernard Montgomery gets deservedly bad marks for conduct but Alan Francis Brooke is rewarded a little over-generously for effort. William Slim should have been placed at the top of the class, Harold Alexander at the bottom.

Whereas Germany had no proper unified command system and Hitler dealt with each service separately, the British had an effective committee of the chiefs of staff that worked reasonably well with Churchill, especially after Brooke replaced John Greer Dill in November 1941. Perhaps too great a contrast is made between the chaotic German system and the efficient teamwork of the British. German generals were able to stand up to Hitler on occasion, although it was a nerve-wracking experience, and the British chiefs of staff were driven to distraction by Churchill's willful interventions, impatience, and lack of consideration for his allies and associates.

The discussion of British and German strategy is also a trifle jejune. To say that Hitler had very vague ideas based on notions of *Lebensraum* and race without some discussion of the vast literature on his war aims is as unsatisfactory as the assertion that after 1941 the British had a clearer strategic concept. This overlooks the endless squabbles concerning the Mediterranean strategy and the second front, to say nothing of the wrangles over priorities in Asia.

When it comes to a description of how these strategies were put into effect we are treated to little more than brief, though highly competent, accounts of various British and German campaigns that amount almost to a short history of the war. Inevitably in such an endeavor there are some questionable statements. Plan Yellow was never, in any of its versions, a type of Schlieffen plan, nor was it ever given the new code name of "*Sichelschnitt*." The British reluctance to intervene in Finland was not due to the determination of the Swedish to remain neutral or their fear of a war with the Soviet Union, but because they were convinced that the Finns would soon be defeated and that therefore any aid given to them would be lost. There seems to be some confusion about the proposed declaration of union between Britain and France. Was it a desperate move, a cynical example of British imperialism at its most perfidious, or a remarkably generous offer to an ally? The sinking of the French fleet, which caused such terrible loss of life and which placed an enormous strain on Franco-British relations, is recounted in a curiously bloodless manner. That the Allied landings in Sicily were badly bungled is not felt worthy of mention. The "Point-blank" directive designated the crippling of enemy fighters the intermediate objective, but the strategic objective was the destruction of German industry.

Arthur "Bomber" Harris, who ignored the directive, continued with his campaign of terror bombing, the moral implications of which and frightful cost to the RAF are underplayed. Although it is true that Hitler did not give a fig for his allies, apart from a certain concern for Benito Mussolini's well-being, the Allies were hardly as cordial as is suggested here. In fact the Grand Alliance existed only in Churchillian rhetoric and, were it not for the skill, patience, and dedication of Dwight D. Eisenhower, very little positive in the way of Allied cooperation would have been achieved.

The book falls awkwardly between two stools. It is neither adequate as an account of contrasting approaches to decision making, nor is it satisfactory as a short history of the war. Its conclusions are hardly remarkable: intelligence is important in warfare, one nation's minor theater was not necessarily the other's, and Allied campaigns were more varied and showed a greater degree of interservice cooperation. Democracies, we are told, can fight wars, although Mahatma Gandhi seems a rather odd example of this truth. But perhaps it could be added that dictators are not bad at it either. It is suggested that "The German hare started much faster, but the plodding British tortoise eventually won," but perhaps it should be mentioned that the poor old tortoise did get a little help from the Russian bear and the American eagle.

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ANNE DEIGHTON. *The Impossible Peace: Britain, the Division of Germany, and the Origins of the Cold War*. New York: Clarendon Press of Oxford University Press. 1990. Pp. viii, 283. \$34.00.

Anne Deighton's carefully documented "post-revisionist" study of the division of Germany is an excellent addition to the growing literature about Britain's role in the Cold War. Deighton argues that British Foreign Office officials had "a clear ideological inclination . . . to perceive the Soviet Union as both untrustworthy and expansionist" (p. 234). Ignoring evidence of an initial Soviet interest in great-power cooperation, Britain led the way in creating what would soon be called containment policy. But to put their strategy into effect, the British needed to convince the United States to sustain an "interest in Europe" (p. 6) and Britain's position as a world power. Since American policy makers remained suspicious of British actions and open to a settlement with the Soviets, for whom sympathy also remained strong inside the Labour Party in Britain, the Foreign Office adopted a "dual policy." "The appearance of great power co-operation was publicly maintained, but the remorseless focus of British policy was directed to securing an effective Western alliance to contain Soviet might in Germany, in Europe, and throughout the world" (pp. 6-7).

Assumptions by the Foreign Office about the ex-

pansionist nature of the Soviets "contributed in large measure to a reluctance to work seriously with them in Germany" (p. 234). By early 1946, British policy makers were moving to divide Germany. A year of delicate maneuvering, designed to maintain a public image of cooperation while privately trying to convince the United States to take a harder line with the Soviets, climaxed with the issuance in February 1947 of what Deighton calls the "Bevin Plan." The effect of this plan was "clearly to divide Germany . . . for the sake of Western interests and lay the blame at Moscow's door" (p. 124).

Although the focus of Deighton's detailed monograph is narrow, it has broad implications for our understanding of Britain's role in the Cold War. It confirms what other recent studies have argued: that Britain, not the United States, took the initiative in creating a Western bloc and dividing Germany. By showing that British policy was set above all by the mandarins of the Foreign Office, and by emphasizing the extent to which Foreign Secretary Ernest Bevin misled the British public and his own followers in the Labour movement about government policy, Deighton also confirms many of the suspicions held by the Labour Left at the time and calls into question studies of the Cold War that uncritically accept Foreign Office justifications for British actions. Most provocatively, Deighton's study suggests that the British deliberately helped to undermine a possible Soviet-American rapprochement because they thought "such collaboration could have left Britain herself an impoverished second-rank power, without a world or European role" (p. 235).

Because Deighton's interesting monograph is based almost entirely on British documents, its vision of events is necessarily one-sided. The Soviet view of these developments cannot be told with certainty until their archives are opened. But the American side may require some corrective. It often appears from Deighton's account that the United States was simply the object of a highly successful British policy. This conclusion seems open to question. Britain may have taken the initiative, but the United States had the power and the cash, and its goals were the ones Britain ultimately had to accept.

PETER WEILER
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MICHAEL MACDONALD and TERENCE R. MURPHY. *Sleepless Souls: Suicide in Early Modern England*. (Oxford Studies in Social History.) New York: Clarendon Press of Oxford University Press. 1990. Pp. xvi. 383. \$89.00.

Jointly researched by Michael MacDonald and Terence R. Murphy but written by MacDonald, this book builds on the authors' recent published articles—particularly MacDonald's "The Secularization of Suicide in England, 1660–1800" (1986) and Murphy's

"'Woful Childe of Parents Rage': Suicide of Children and Adolescents in Early Modern England, 1507–1710" (1986)—supplementing them with new materials and revising earlier conclusions. What emerges is a skillful and forceful study not only of suicide but also of the cultural, social, and political history of early modern England. For MacDonald and Murphy, "the importance of the history of suicide lies in what it reveals about the nature of cultural and social change" (p. 338). Throughout their book they have used attitudes toward suicide as a vehicle to analyze and amend historians' views of the forces that shaped power relations in early modern England. For instance, their examination of the transformation of suicide from a diabolical crime to a secular event forces a reevaluation of the two-class model of social and cultural change. The authors demonstrate that any explanation of the secularization of suicide must acknowledge the crucial role of "the men of middling status who served as jurors" (p. 344). If the middling sort influenced such deep-seated popular and official values as those attached to suicide, historians must consider the implications for other political and cultural developments.

The book is divided into three sections. The first traces the "intensification of hostility" (p. 5) to suicide during the Tudor revolutions. Increasingly, strict enforcement of laws calling for confiscation of suicides' chattels and of the rites of bodily desecration paradoxically "concealed a deepening ambivalence toward suicide, expressed in the behaviour of local communities and the lay educated élite, that was generated by social, political, and intellectual changes" (p. 106). This dialectical situation helps us comprehend the rapidity of the decline in severity toward suicides at the beginning of the seventeenth century.

The second section focuses on the secularization of suicide, connecting it with the abandonment by educated laypeople of supernatural explanations of all kinds and with the fears of church leaders of the growing appeal of enthusiast sects. Although belief in the diabolical roots of suicide continued to resonate among lower sorts and evangelicals, the decline of savage penalties seemed irreversible by the reign of George III.

The final section moves from changing attitudes toward suicide to an examination of the suicidal themselves. The authors' identification of young people and the elderly as those at greatest risk has interesting implications for current experts' proclamations attributing the rise in youth suicide to the complexity of late-twentieth-century existence. "The motives that observers and suicides gave for self-killing," write MacDonald and Murphy, serve as "an index of what contemporaries were afraid most to lose" (p. 298). Although some of these motives reflect the peculiarities of the early modern era, they nevertheless have much in common with issues that appear to inform contemporary suicide: family, economy,

and shame. Finally, MacDonald and Murphy explore the impact of the print media on attitudes toward suicide as well as on behaviors of the suicidal themselves. Using suicide as their emblem, they conclude that "the rise of the popular press and the common people's mastery of its rhetorical conventions changed forever the relationship between the individual and society" (p. 337). In short, this book makes an important contribution both to the history of suicide and to what the study of suicide can reveal about history.

HOWARD I. KUSHNER
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LEO F. SOLT. *Church and State in Early Modern England, 1509–1640*. New York: Oxford University Press. 1990. Pp. xii, 272. \$35.00.

In five chapters on "The Henrician Schism," "The Anglican Reformation," "The Elizabethan Challenges," "The Jacobean Consolidation," and "The Laudian Counter-Reformation," Leo F. Solt surveys the shifting relationships among the monarchs, Parliaments, and leading clerical spokesmen of England during the sixteenth and early seventeenth centuries. Especially sensitive to claims for the "*potestas ordinis*—the sacerdotal power . . . to deal with" a person's "inner spiritual relationship with God"—and the "*potestas jurisdictionis*—the power of ecclesiastical administration"—made by various parties during this period (p. 25), Solt provides an informed, well-organized, easily read synthesis based on a critical analysis of a wide selection of specialized historical accounts and a limited body of sources. Although reaching out in places to embrace events in Scotland, the Netherlands, and New England, and making some reference to the Continental impact on Henrician, Edwardian, and Elizabethan reformers, this remains a largely insular account.

The opening chapter surveys the jurisdictional relationship between the kings of England and various popes during the late Middle Ages before dealing judiciously with the legislation of religious changes by Parliaments and convocations under Henry VII. Although this account displays a good sensitivity to the limits of the Henrician reformation, only brief references to the attack on relics, images, and pilgrimages (the core of traditional piety) and the concurrent new piety enforced by the royal injunctions of 1536 and 1538 blunt the Protestant aspects of the Reformation of the 1530s. Discussion of the Reformation under Edward VI deals judiciously with the change to a vernacular service and the replacement of altars by communion tables, but neglects other important reforms such as the marriage of priests. Exiles to Germany and Geneva and martyrs receive the largest share of attention for Mary's religious regime. Discussion of the acts of Supremacy and Uniformity of 1559, the Injunctions of 1559, and the Thirty-Nine

Articles takes account of the recent work of Winthrop Hudson and Norman Jones and interprets this settlement as "a lopsided compromise on matters of Church worship and doctrine between conservative and moderate reformers in the Anglican Church" (p. 79).

The chapters on the Elizabethan and Jacobean church pit "The Puritan Threat" and "The Catholic Threat" against the "English Church and State"; anachronistic "Anglicans" confront "Puritans" in a manner difficult to sustain after the publication of Patrick Collinson's *The Religion of Protestants: The Church in English Society 1559–1625* (1982). Attempts to contain the findings of recent studies within an old-fashioned interpretation lead to interpretive difficulties, as does the Whiggish claim that disputes over the judicial power of the church led Sir Edward Coke and John Selden to join "the parliamentary opposition" (pp. 156–57). These chapters, however, contain many valuable pages, including balanced introductions to the "covenant theology" favored by early seventeenth-century English Protestants and to English participation in the Synod of Dort.

The Caroline chapter stresses recent concerns with the changes attempted by those "anti-Calvinists" who received the blessing and patronage first of Bishop Richard Neile and then of Archbishop William Laud. Solt, however, too easily turns those critics who claimed to uphold the traditions of the Elizabethan and Jacobean Church of England against "innovations" of the Laudians into radical "Puritans." Readers need to know that Laud's definition of "Puritan" would have embraced the vast majority of the Elizabethan and Jacobean archbishops and bishops, including Archbishop John Whitgift, and that the "ill-affected" men mentioned by Charles I in 1640 included not only the "radical and Separatist Puritans" noted here (p. 202) but also such moderate defenders of Elizabethan and Jacobean practice as Bishop John Williams and Sir Henry Marten, Dean of Arches.

Although this survey provides a reasonable introduction to a vast and complex subject, the last three chapters remain too encased in an old-fashioned contrast between a widely defined "Puritan" movement and a narrowly defined "Anglican" church to provide a sympathetic guide to much of the recent historiography on the subject.

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LAWRENCE STONE. *Road to Divorce: England, 1530–1987*. New York: Oxford University Press. 1990. Pp. xxvii, 460. \$29.95.

This volume provides an introduction to two forthcoming volumes of case studies drawn from Lawrence Stone's researches into the ecclesiastical

court records from the province of Canterbury: *Uncertain Unions*, about "the making of marriage" before the Marriage Act of 1753, and *Broken Lives*, about "the breaking of marriage" before the Divorce Act of 1857. In the present work he gives general accounts of formal and informal ways of contracting marriage, problems establishing the validity of marriages, separation by decree in the ecclesiastical court and separation by private contract, the tort action for criminal conversation, and parliamentary divorce and parliamentary change in the divorce laws. The only topic that seems oddly slighted, given the focus on divorce and his fondness for a good story, is the ecclesiastical declaration of nullity, that is, the declaration that a marriage had never existed. Perhaps the subsequent volumes will tell more of marriages voided for incest or physical incapacity. Acknowledging that the years of his primary focus, 1660–1857, constitute merely the "prehistory" of divorce (p. 28), because minuscule numbers of persons then obtained separations or divorces, he nevertheless considers that fundamental changes in mentalities occurred to prepare the way for the more recent avalanche of divorce. Stone's leitmotifs here are reminiscent of those in his famous *Family, Sex, and Marriage in England, 1550–1800* (1977): the rise of secularism, more equality for women, and the development of a pleasure-seeking affective individualism, until, finally, "the ideology of individualism" triumphs "over the ideology of social responsibility" (p. 15).

Stone is impressed by the apparently high level of confusion and "corruption" in this system of marriage law and by the self-interestedness of its practitioners. Indeed, he seems deeply ambivalent on the question of whether the system of marriage law was functional. On the one hand, he expresses ideas of functionalism often used in the older legal history. On the other hand, he repeatedly discovers dysfunctionality and unresponsiveness, even to the wishes of the elite, to say nothing of the humbler. Both conflict of laws between competing jurisdictions and the vagaries of particular rules and their administration, he concludes, made marriage law in practice "a mess," confused entitlements to property, and seemed "to maximize . . . misery," making it "impossible to advance a plausible functionalist interpretation" (pp. 135–37).

Stone's capacious survey goes over much ground that will be familiar to those who have read the specialized work of scholars such as Sybil Wolfram and Mary Lynn Shanley, but he adds useful observations and provides a broader and very accessible account for more general readers. We also get statistical tables about rates and kinds of litigation in the ecclesiastical courts and just enough teasing detail to whet our appetites for the subsequent volumes.

Despite Stone's shying away from some of the deeper questions about the relationship between marriage and structures of production and reproduction and economic and political power, raised by feminist

scholarship, he seems slowly to be warming toward feminist perspectives. He expresses himself feelingly on the stories of women's suffering that his research reveals, some worthy to rank with the atrocity stories reported by radical feminists. We read, for instance, of "a jealousy-crazed husband" who "sewed up his wife's labia" (p. 201). "Even more striking than the ferocity of some husbands," he finds, was the power of the "culture of female submission" and "the passive endurance of so many abused wives in the eighteenth century" (p. 199). Victorianists may be surprised to read that 1857 figures as such an important waystation to what feminists may be surprised to read is "the legal and psychological equality in marriage which exists today" (p. 13), but everyone will find Stone's narratives of matrimonial catastrophe worth pondering.

SUSAN STAVES
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ANN KUSSMAUL. *A General View of the Rural Economy of England, 1538–1840*. (Cambridge Studies in Population, Economy, and Society in Past Time, number 11.) New York: Cambridge University Press. 1990. Pp. xiv, 216. \$39.50.

Economic history, it is widely held, must be bedeviled by a chronic lack of data and quantitative information. As we go back into the precensus years, any attempt to produce quantitative data allowing economic historians to use evidence in modes other than "for example" must be doomed. This view, most widely held by people who are not active in the field, is mistaken. There is far more quantitative evidence available than nonspecialists suspect; the trick is to see where it is and how to use it. New Economic Historians have ingeniously learned to use sources for purposes that they were never intended for in order to produce new quantitative information and evidence. They have, in addition, used their computers to analyze the data every which way, squeezed every drop of information from the numbers, and then agonized long and hard over the many biases and limitations in these sources. In that process the historians have learned and taught many new insights into the past.

In this little book, Ann Kussmaul provides us with a brilliant example of exactly such a project. It is based on a clever and basically simple idea. Consider the question of the occupational and agrarian structure of Britain before the Industrial Revolution. On the surface, this might seem like one of those "sorry, no data" kind of issues. But Kussmaul realizes that the demographic data bank collected by E. A. Wrigley and his collaborators could be used to shed light on this issue as well. Her insight is to use the fact that economic structure determined the season in which people married to uncover how these people made a living. She notes that wedding ceremonies were ex-

pensive in terms of time. Consequently, in arable areas weddings would be postponed to later in the fall, whereas in largely pastoral areas they would be postponed until late in the spring. In areas of rural industry, seasonality would be weak or nonexistent. Modern statistical techniques and computers can pick up these patterns from her collection of 542 parishes and classify them by predominant marriage season as pastoral, arable, or rural-industrial. These patterns are combined to create a "general view" of Britain, a magnificent if somewhat blurry panorama of cross-sectional and time-series observations of the changes in agrarian structure, providing the kind of information that was not imaginable even twenty years ago. It is truly new light cast on a statistical dark age.

The historical conclusions that Kussmaul draws from her new view confirm the hypothesis put forward more than twenty years ago in a classic paper published by Eric L. Jones ("Agricultural Origins of Industry," *Past and Present* 40 [1968], 58–71). In the second half of the seventeenth century the British rural economy underwent a process of regional specialization that created three distinct zones: a pastoral area, a grain-growing region, and a rural-industrial region. The seventeenth century, in this interpretation, provided the key ingredient to subsequent economic growth, namely, consistent "Smithian" growth, the gains from trade and interregional division of labor according to comparative advantage. By locating these changes squarely in the seventeenth rather than in the eighteenth century, Kussmaul has placed a challenge to those who are looking for direct links between agricultural and industrial growth in the Industrial Revolution. She is thus able to demonstrate a new timetable of the growing commercialization of the British agrarian economy after 1600 and to show how Smithian growth could have affected living standards.

The book, pioneering as it is, does not make for very good reading. Although Kussmaul tries her best to liven up her prose, the work is inevitably laced with tables, graphs, and maps. The New Economic History may never become literature. But her work shows how economic history is able to advance through the combination of better computers and the ingenuity of its practitioners. Some of the complicated graphs (Kussmaul clearly has a preference for pictures) are a bit confusing, and Kussmaul seems to forget from time to time her own warnings on the arbitrariness of her classifications and the proximate nature of her marriage seasonality variables, driving her analysis a bit beyond the boundaries of caution. Yet such minor quibbles should not distract from an effort that is as original as it is clever. There are unmined data treasures in economic history, if we only know where to look.

JOEL MOKYR
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CONRAD RUSSELL. *The Causes of the English Civil War*. (Ford Lectures Delivered in the University of Oxford, 1987–88.) New York: Clarendon Press of Oxford University Press. 1990. Pp. xv, 236. \$84.00.

Conrad Russell here summarizes and gives argumentative focus to his much longer *The Fall of the British Monarchies, 1637–1642* (1991). The result is a quirky book, written in haste (Russell testifies to the pressure of his publisher), yet driven by a powerful central theme and illuminated by flashes of brilliance.

Elements of Russell's case are familiar. There was no great constitutional crisis, and Russell points to a few senior lawyers who supported the Crown in 1642 and to the many royalist MPs who had in 1628 backed the Petition of Right. There was a religious crisis, but the anti-Calvinist and ceremonialist policies of Charles I and Archbishop William Laud alarmed many besides the fiercely anti-popish few. And there was a fiscal conundrum, since Tudor conventions of supply had quite conspicuously not kept abreast of the rising costs of war. But dissidents could and did emigrate, while Charles found considerable legal support for ignoring fiscal convention in order to defend the land. None of this sufficed to bring down the monarchy.

What proved catastrophic was the Stuarts' British, not their English, inheritance. James I had carefully removed the worst bones of principle that might stick in the throats of his various subjects. But as the troubles over the Scottish Prayer Book showed, Charles was oblivious to the risks in ruling multiple kingdoms in an age of budgetary inflation and religious intolerance. The Covenanters' revolt crippled him, just as the revolt of the Irish Catholics in 1641 finally destroyed his polity. Russell weaves a compelling story out of the transnational agendas and alliances of the various protagonists in the three kingdoms. No party was strong enough to pursue its objectives alone, while each could be threatened by developments elsewhere. The result was disaster, for trust, elusive enough in one capital city, could not envelop three.

Russell's argument for the central role of the British problem in bringing down the Stuart monarchy is surely unchallengeable. There is much to learn here, too, of the independence of courtiers and councillors, and of the varying relations between hotheads and constitutionalists in the Commons and politiques in the Lords. But students cannot be referred to this book without caution. Not only are its lecture origins too apparent in its allusiveness and repetitiveness but it is also carelessly produced, with errors and infelicities to be expected of neither author nor press. And judgments are often over-simplified, as in Russell's use of Sir John Eliot's comments on figures provided by his enemy Sir John Coke to prove the Commons' financial obscurantism. Indeed, Russell ignores challenges to his earlier arguments about the relationship between war and parliaments in the 1620s; his excul-

pation of Charles therefore stands on shaky ground. Russell takes Charles's debt repayments in 1642 as rational preparation for new borrowing; but a king who repaid women suppliers for past royal galas looked more to honor than political arithmetic, fitting older interpretations more than Russell allows.

When Russell concludes, apropos Covenanter pressure on Parliament for presbyterianism in early 1641, that "the Scots had not reformed England but divided it," he demonstrates the interweaving of the three kingdoms (p. 125). But he also highlights his relentless focus on high politics, his assumption that division emanates from the center, and his questionable conviction that the English were inherently consensual. A work that pays such short shrift to recent scholarship on the localities, and on London above all, is unlikely to persuade readers that the causes of the civil war can be crammed into a five-year span.

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GEORGE HILTON JONES. *Convergent Forces: Immediate Causes of the Revolution of 1688 in England*. Ames: Iowa State University Press. 1990. Pp. xvi, 218. \$29.95.

George Hilton Jones tells his readers exactly what he intends to do in this book: narrate both the immediate causes of the English Revolution of 1688 and the conciliatory steps King James II took to thwart his enemies at home and on the Continent. By "immediate causes" Jones means what happened over a period of one year from the fall of 1687 to the invasion of England by Prince William of Orange in the fall of 1688. Jones also tells us precisely how he intends to proceed. Embracing a Rankean ideal, he wants to relate "as thoroughly as the availability of contemporary sources" allows, "what happened, who did it and their immediate reasons" for their actions (p. vii). Admitting that a single chronological narrative would "confuse" the reader, Jones chooses to arrange his evidence in ten "parallel narratives" that cover in eight chapters the trial of the Seven Bishops, the birth of King James's son, the recall of British soldiers from the Dutch service, and Prince William's success in winning support in the Netherlands and with the English opposition, and in two chapters King James's efforts to curb the erosion of his support and avert a military encounter with William. The details about the men who appear are intended to heighten the reader's perception of what Jones calls, without further explication, "the texture" of history (p. vii). Another provocative comment, also left unexplored, is Jones's description of his book as a "depiction at length of [James II's] personality" in 1688 and of "its consequences" (p. xiv). Jones does not practice psychohistory, but he is clearly fascinated by the king, as is suggested by scattered remarks on his arrogance, stubbornness, and "reliance on Providence and his

own authority," described as "greater than the rational mind can grasp" (p. 120). Jones could have enhanced his book by drawing together his insights into a word portrait of James.

The strengths of this book lie in the meticulous research Jones has undertaken in English, French, Dutch, Italian, and German archival and printed materials. Every reader and not just the "discerning" reader (as Jones hopes) will recognize how skilled and indefatigable he is as a researcher. The result is the recovery of unknown or little-known details about these months: for example, the names of Anglican clergymen who read James's Declaration of Indulgence as required (pp. 15–16, 20) or the recognition by D'Avaux, the French Ambassador to the Netherlands, that Louis XIV had blundered in his policy regarding that country in 1688 (pp. 108–14). Another feature is Jones's locating the English Revolution in an international context; the policies and interests of William, James, Louis, the Estates-General, and Dutch cities, particularly Amsterdam, were each important to isolating James and making William's invasion of England possible. Also admirable are Jones's shrewd comments, dispersed throughout the text, on persons and policies (for example, pp. 154, 163).

Jones's concept of the historian's craft, however, does not serve him well in my view. Reconstructing the events of a year in a larger story can have much merit; historians of the English Civil Wars and the French Revolution have done as much. But Jones seems, unknowingly, to be a captive of his prodigious research. The "availability of contemporary sources" alone does not determine a narrative; rather, it is what the historian looks for, decides is important, and interprets. For example, the Bishops' Proposals of October 3, 1688, deserve analysis. They were a sign of weakening ties between Anglicans and Dissenters, and James's failure to respond to them reinforced his obtuseness. James's efforts in the fall of 1688 to rehabilitate himself included the use of the press; the fact that the mob razed the house of the king's printer suggests effectiveness. Moreover, this book suffers from failure to articulate a thesis, identify the points of intersection between and among the "convergent forces," and draw conclusions from the whole. The introductory pages do not discuss the problem of the book—the causes of the revolution—nor does the one-and-one-half-page conclusion. In other places Jones does present interpretive comments, noting that "European policy" mattered most to William but very little to his English friends (p. 120), stressing that Louis's policies toward the Netherlands ruined the French party there (p. 161), and underscoring throughout James's insensitivity. These and other points need to be integrated into a coherent statement offering the author's conclusions about the "convergent causes" of the English Revolution.

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PETER EARLE. *The Making of the English Middle Class: Business, Society, and Family Life in London, 1660–1730*. Berkeley and Los Angeles: University of California Press. 1989. Pp. xiii, 446. \$35.00.

James I said that “soon London will be all England.” This timely and well-conceived study of London business life in the age of E. A. Wrigley’s “simple model” helps gauge the truth and implications of that statement. Analyzing 375 estate inventories and their disposition by the London Court of Orphans and a rich array of other sources, Peter Earle ably supports his general contention that “the period 1660–1730 saw fundamental changes in the lives of the middle class . . . which have never been reversed and lead inexorably to the even more middle-class middle class of the nineteenth century and today” (p. 334). As this candid engagement with J. H. Hexter’s perennially rising universal bogey suggests, the book is engagingly written. It does not strain the evidence, treats moot points with common sense rather than tendentious speculation, and is clearly presented so that the reader retains a sense of contact with the people whose lives are its subject.

Although they were largely recruited by apprenticeship from the younger sons of gentry families and were not explicitly set apart from county society by any conscious rejection of aristocratic manners or morals, the commercial gentry of London was already distinctively urban. In wealth and range of occupation they varied as much as the economy of the capital itself. Despite such internal variety, what distinguished them collectively from other social groups was that they lived by profit on the turnover of capital that they themselves for the most part formed and accumulated. This investing community invested in itself, mostly through business loans and other movable instruments necessary to the flow of commerce. Only late in the period did most of its members begin to hold long-term government stock. The community’s real property, if rural, was mostly inherited; if purchased it was predominantly urban and suburban. Current research over a longer time span using a larger and differently derived sample may modify this profile. As it stands, however, it supports the revisionist contention that bourgeois entry into the landed elite was slower and less sought than has been traditionally assumed.

The picture that emerges from Earle’s study is of a social group with distinctive characteristics. The most essential derived from the requirements of cash flow and accumulation: the interconnection of domestic and business finance, which mirrored the equally close association of household and workplace, and the nexus between capital formation, family formation, and the transmission of wealth from one generation to the next. Marriage was the prime chance to gain extra business capital, but for that very reason portions and settlements were seldom tied up in fixed forms. Settlements were rarely made by jointure, as

among the gentry proper, but instead took the form of commitments against the husband’s future income or estate. These differences were reinforced by other factors. The surplus of females in the population meant that middle-class London women tended to marry younger. For men who could afford one, a young bride not only sweetened the prospect of capital gain but also offered the most lasting combination of domestic comfort and trainable business support, especially since the low price of hiring servants minimized the opportunity costs of pregnancy and child care. This had two results: first, the emergent notion of “separate spheres” and the first deplored signs of the kept wife; second, the businessman’s widow, most likely to be a *rentier*, creditor, or investor, but often in trade herself as *feme sole merchant* under the Custom of London, in which case she was probably in catering, haberdashery, or pawnbroking.

The fundamental factor in middle-class life remained, however, the direct relationship between prospects for accumulation and life expectancy. Better living conditions and the retreat of epidemic diseases caused mortality among middle-class young and middle-aged adults to fall rapidly after the 1690s. Longer productive life spans meant a cumulative increase in the wealth that each generation could pass on. The business community’s ability to reproduce itself culturally grew accordingly, as sons increasingly followed their fathers’ footsteps. Nevertheless, at the level of the individual family, sickness and death still threatened. Although Earle rightly stresses both the importance of qualitative rather than quantitative changes in the lives of middle-class Londoners, and the humane completeness rather than the mere pecuniary efficiency of their lives, some sense of severity and constraint remains. In the debate over the family and its internal relations, Earle’s position is closer to that of Lawrence Stone and Jeanne Stone than to their critics. Similarly, the most revealing aspect of the cultural self-making, expressed in the consumption patterns of middle-class Londoners, is neither its impact on aggregate demand nor simply the emulation of aristocratic taste in pursuit of “true comfort.” Instead, it is the fact that, beyond a certain point, these people did not try to mimic those above them. Much room existed for individual fastidiousness in pursuit of a self-image appropriate to status and wealth. Collectively, however, this community’s increasing prosperity was more evident in the upgrading of standards among its middling and lesser members than in conspicuous display at its upper levels.

Earle’s title alludes to another famous “making,” now nearly thirty years old, and his preface, while different in purport and addressed to a very different world, contains some hints of an analogous historiographical manifesto. For all their obvious differences, the two works are worth comparing. Like E. P. Thompson, Earle speaks for England as a whole but does so from one predominant place. He therefore

begs similar questions: is the singular case in his title quite appropriate, and does London in truth stand for the country as a whole? As his acknowledgment of his Londoners' country connections implicitly recognizes, it is not certain that their world was exactly reproduced elsewhere, for by the time later in the century when provincial developments reached a comparable scale the structure of the economy was different from what it had been, and becoming more so. Another important comparison might be made. Like Thompson, Earle is concerned with class as lived experience, especially as self-making. His sources convey this via material culture: consumption made possible by some degree of affluence. In Earle's case, therefore, the components of the construction are very different from those examined by Thompson. Moreover, on individual sensibilities and particularly on religion, Earle's sources are mostly silent. His account therefore strikes a contrast not only with Thompson but also with Catherine Hall and Leonore Davidoff's study (*Family Fortunes: Men and Women of the English Middle Class, 1780–1850* [1987]), based on Birmingham and East Anglia, of the English middle class in its provincial setting just over a century later. If this counterpoint suggests that there is more making to be uncovered, however, it is nevertheless true that Earle has established a substantial benchmark from which to begin.

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PAUL KLEBER MONOD. *Jacobitism and the English People, 1688–1788*. New York: Cambridge University Press. 1989. Pp. xvi, 408. \$49.50.

In recent years Jacobitism has emerged as a centrally important issue in the historiography of eighteenth-century England. Once dismissed as a manifestation of nostalgia, the movement is now often invoked as the key to understanding the religiosity, politics, and social conventions of the era. It is a subject that has cried out for a chronicler. Paul Kleber Monod has accepted the challenge with a success that will reward all who study the era.

Perhaps the most extraordinary feature of Jacobitism was its durability, for it arose as a consequence of the alteration in the royal succession attendant on the Glorious Revolution and continued to influence English politics four reigns later. During that span of years, there were three surges in Jacobite sentiment. In the first decade after the arrival of William and Mary, Jacobitism flourished among those who had been influenced by the Toryism of the 1670s and 1680s and who hoped for a quick undoing of the new reign. In the decade that opened with George I's accession, it reemerged as a vehicle for those defenders of the Church of England who despised the Whigs' rejection of the Henry Sacheverell settlement.

In the heady days of the mid-1740s, Jacobitism pulsed yet again when the possibility that French arms might oust Hanoverian corruption stimulated a return to forsaken loyalties. A commitment that could challenge the throne of William III and threaten the career of Henry Pelham was neither an ephemeral nor an inconsequential phenomenon.

Throughout its lifetime Jacobitism provided the matrix for an identifiable subculture within British society. Adherence to its beliefs might affect the way in which persons decided to dress and their choices of clubs, schools, and hobbies. And yet Jacobitism, far from being a monolith, was an aggregate of "communal solidarities," among which ex-army officers, Irish immigrants, Nonjurors, and English recusants figured prominently.

Monod acknowledges that Jacobitism had both its shock troops and its trimmers—and varieties of adherents in between. As a consequence he is cautious in assessing the demographic breadth of the movement. He concludes that Jacobitism, although the most widely significant form of seditious thought in eighteenth-century Britain, was not the dominant cast of mind of the era; that nevertheless a commitment to Jacobitism energized the most extensive outbreak of civil disturbances (1714–15) that the century witnessed; and that (as a broad guess) perhaps one-quarter of the national elite were subscribers to its doctrine.

Not until the second half of the eighteenth century did Jacobitism cease to pose a threat to the Hanoverian scheme of order. It receded because—as the epitaph of one of its adherents says—"the times changed, but he did not change with them" (p. 306). Its legacy was a peculiarly complex one: its structures of action supplied a framework for extraparliamentary radicalism, whereas its structures of conviviality provided a pattern for the elite sociabilities that came to define conservative defenses against change. Monod argues for these points—and many others—with admirable clarity and an enticing array of evidence.

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BRIDGET HILL. *Women, Work, and Sexual Politics in Eighteenth-Century England*. New York: Basil Blackwell. 1989. Pp. viii, 275. \$39.95.

Bridget Hill's book is a solid, substantial work, thoroughly researched and generally well written, which makes important contributions to our knowledge of the Industrial Revolution, women's history, and the eighteenth century. With this volume Hill realizes her laudable goal of re-integrating women's history with social history.

The book's major contribution is to show women's participation in preindustrial economic life. Women were productive workers engaged in a wide range of

economic activities. They were significant actors in what has come to be called the family economy. Hill shows quite convincingly that the enclosure movement not only had an impact on women workers but probably affected them more than it did men, with dire consequences for the family economy as a whole. Changes in farming methods further marginalized women as agricultural producers. Simultaneously, the decline in apprenticeships closed off other avenues of economic endeavor. Thus, even in the earliest years of the agricultural revolution, women were increasingly excluded from active participation in the economy, making families more dependent on the labor of adult males, creating greater gender stratification of the work force, and leading to the feminization—and invisibility—of housework. Hill argues, however, that much of this loss was made up by the growth in domestic service.

Not surprisingly for so ambitious a work, this book is not without its flaws. The major problem is the lack of an analytical or theoretical framework that would have tied the chapters together better. As it is, each chapter can stand on its own, a series of discrete facets without any real development. That this is a weakness is most evident in the chapters on women in the family. There is no attempt to relate the personal to the political, or gender roles to economics, beyond discussions of the declining family economy. Sexual politics is precisely what we do not get. (The title “Women, Work, and the Family Economy” would have given readers a better idea of what to expect.) Although Hill suggests that the decline in women’s productivity should have left them in a weakened situation within the family, she presents no evidence that this was so. There are no sustained discussions of prostitution or sexual vulnerability at work, the sort of issues that might have led to some theoretical linkage. Indeed, although Hill admits she did not have space to include prostitution, it is an omission that seems glaring, particularly in her chapter on casual, migratory, and other forms of “invisible” employment.

There also seem to be smaller areas of confusion that erode a reader’s confidence. Sometimes Hill’s evidence seems to contradict her arguments. For instance, Hill tells the story of an orphan who chose to go into service over the objections of relatives who wanted to apprentice her to a milliner (p. 143). The family regarded the orphan as having “lost caste” through this unfortunate choice. Nevertheless, in the same paragraph, Hill asserts that parents regarded domestic service as a “desirable occupation for daughters” because they saw it as the means of “social betterment.” Since she follows this line of argument elsewhere (p. 99, for instance), one has to question Hill’s conclusion that the substitution of domestic service for agricultural work represented a decline in women’s economic status. Similarly, Hill argues that apprenticeships, particularly in such traditional fe-

male fields as millinery, were prestigious. “For aspiring parents of the middle class,” she writes, “these trades represented some possibility of social advancement for their daughters” (p. 95). Given that this statement contradicts our usual assumptions about class and gender, it requires a great deal of evidence, elaboration, and precision of definition to back it up. In the absence of these, a great many questions are being begged.

These weaknesses are unfortunate, because Hill’s book is generally a sound volume that merits our attention and interest. It should prove a useful synthesis of the materials that have become available since Pinchbeck’s time.

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JAMES E. BRADLEY. *Religion, Revolution, and English Radicalism: Nonconformity in Eighteenth-Century Politics and Society*. New York: Cambridge University Press. 1990. Pp. xxi, 473. \$49.50.

The thesis of James E. Bradley’s new book is that in the “generation” between the mid-1760s and the mid-1780s, Dissent—in some of the boroughs in which it had a sizable presence—became the spearhead of a vital, popular radicalism, with the preaching of Nonconformist ministers a critical influence. To make his case, Bradley provides an analysis of five provincial Nonconformist ministers’ sermons and then deploys for ten boroughs of varying size and structure the psephological techniques of John Phillips’s *Electoral Behavior in Unreformed England* (1982) to examine contested parliamentary elections (along with the sizable body of extant electoral literature for some of his locales) and the petitioning campaign in 1775 against the North ministry’s American measures that mobilized many who did not hold the parliamentary franchise.

On these evidentiary bases, Bradley draws the following conclusions. First, the extent to which local political opposition was aligned to national issues and divisions has often been underestimated, with Bristol the most striking case in point. Second, the extent and consistency of Dissenting political activity at the local level was not simply a function of religious affiliation: even in boroughs in which the Dissenters had a significant numerical presence, the operation of electoral patronage and the working of the Corporation Act could sharply limit their role. Conversely, the presence of charismatic leaders (usually, but not invariably, ministers) could make Dissenters a force of greater weight than their numbers or access to local office alone would explain, as in Newcastle, Taunton, and Nottingham. Third, the politically active Dissenters and their “low church” (or at least nominal Anglican) allies display differing socioeconomic profiles: among the former, opposition spanned the existing socioeconomic spectrum, but

the latter were recruited disproportionately from the middling and lesser occupational groupings (see especially table 10.4 on page 400, the penultimate of forty-five tables in this work). Thus, while emphasizing the confessional springs of political choice, manifest also in the conduct of the Anglican clergy, Bradley argues that the many non-Dissenters who signed the petitions of 1775 were probably motivated by socioeconomic discontent. It is not that religious and economic factors were necessarily opposed or distinct; indeed, Bradley goes on to speculate that they were sometimes interactive, with Dissenting elite leadership and Dissenting preaching that emphasized individual moral autonomy and the dangers of encroachment by authority on individual liberty contributing to the political mobilization of non-Dissenting skilled artisans and shopkeepers.

Bradley's findings may be put in historiographical perspective by juxtaposing them to the assertions of Jonathan Clark. Both are convinced that sermons are a key to understanding contemporary political ideology, but Bradley contends that political radicalism was by no means confined to the heterodox among the Dissenting ministers. Moreover, whereas Clark minimizes socioeconomic differences as a basis of political division, discounts the value of psephological research, and concentrates on "high politics," Bradley exploits a wide range of sources and modes of analysis to formulate a carefully argued and generally persuasive account of the interplay of ideas, religious affiliations, and socioeconomic circumstances.

What, then, of the rest of the kingdom beyond these ten boroughs? Clearly, Bradley's analysis is limited in application. Yet, as his work illumines the wellsprings of political behavior in his chosen locales, so his study is fertile in its suggestions for further investigation, in particular on connections between the popular radicalism of the 1770s and the artisanal radicalism of the 1790s. Bradley does not labor alone; together, his book and Frank O'Gorman's *Voters, Patrons, and Parties: The Unreformed Electoral System of Hanoverian England, 1784-1832* (1989) make major contributions to our understanding of the character and behavior of the expanding political nation of later eighteenth-century England.

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ELLIS ARCHER WASSON. *Whig Renaissance: Lord Althorp and the Whig Party, 1782-1845*. (Modern European History.) New York: Garland. 1987. Pp. viii, 439.

The herculean struggle to pass the Reform Bill was Lord Althorp's finest hour. Despite a mediocre speaking ability in an age of parliamentary eloquence, "Honest Jack's" command of the House of Commons was, according to most contemporaries, instrumental in guiding the bill through. This study examines how he earned leadership and trust and his role in the

Whig pantheon. Although published in a dissertation series, Ellis Archer Wasson's book departs from his Cambridge University thesis of 1976, a study of the young Whigs from 1809 to 1830. Wasson now concentrates on Althorp's role as the linchpin of the Whig renaissance of the late 1820s and his contribution to the reform legislation of Lord Grey's governments from 1830 to 1834.

Wasson's treatment of Althorp is similar to Richard Brent's recent study of Lord John Russell and the liberal Anglican Whigs (*Liberal Anglican Politics* [1987]). Both describe the evangelicalism and sense of moral purpose in the thought and development of the younger "Whiglings" (p. 69). Althorp, for whom politics was a duty pleasing to God, was more liberal than the Whigs of his father's generation in his moral opposition to slavery, militarism, and corruption, and in his support for popular economic and parliamentary reform. Yet Althorp was a landowning Spencer whose Whiggism could be traditional. Parliamentary reform was necessary for effective social and economic programs, but the Tory and monarchical attack on local patrician leaders (which almost unseated Althorp as chair of the Northants Quarter Sessions) convinced the future earl that the electoral system needed to be safe as well as effective. Similarly, although Althorp opposed the self-serving Corn Laws, he would not upset landed interests by forcing change that they were not ready to accept. Like most Whigs, he trimmed.

Wasson has drawn on a wide range of primary sources to chronicle the political career of a most reticent and self-conscious public figure. In addition to the archives in the muniments room at Althorp Park, access to more recently available collections of the papers of leading Whigs have allowed Wasson to produce a meticulous piece of scholarship. The chapters on Althorp's early career, and on his involvement in sporting, agricultural, and educational societies, are in many respects the most interesting, a biography not heretofore told.

Wasson's judgments are sound, in most cases similar to those proffered by Austin Mitchell and Leslie Mitchell, Michael Brock, Anthony Brundage, and Abraham Krieger. Occasionally, the tendency to ascribe importance to his subject is more forcefully stated than argued. Was he "the" architect of reunion, "the" progenitor of a new party who orchestrated the renaissance of Whiggery? (pp. 351-52). Was he, rather than Lord Holland, the conscience of Whiggery? Was he as antimonarchical or as ready to join with the movement as this portrait suggests? Could he really have forced himself on the king as premier in 1834? But these are insufficient cavils to reduce the importance of this timely portrait of a Whig who did his duty and then left.

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R. S. FITTON. *The Arkwrights: Spinners of Fortune*. New York: Manchester University Press; distributed by St. Martin's. 1989. Pp. xiv, 322. \$70.00.

Scholars have eagerly awaited the product of R. S. Fitton's years of research and scholarship on the Arkwright family. Single-minded in the extreme, Fitton made the study of the Arkwrights his life's work. His research in economic and business history lay within what could be called the "Manchester tradition," following in the model delineated by T. S. Ashton, George Unwin, Joseph Sykes, G. W. Daniels, Arthur Redford, and A. P. Wadsworth. A distinguishing characteristic of this tradition was an emphasis on empirical study firmly based on primary sources. Fitton was a graduate student of Ashton and the Manchester tradition characterized his first main publication based on his doctoral thesis: *The Strutts and the Arkwrights, 1758-1830: A Study of the Early Factory System* (1958). It was the discovery of a new set of documents (the papers of a descendent of John Smalley, Arkwright's first partner), and other papers of the Arkwright family, which were made available by a direct descendent of Richard Arkwright, that provide the new documentary material for this book. Sadly, Fitton did not live to see the publication of his most substantial scholarly work.

The first Richard Arkwright, that hero of the British Industrial Revolution and pioneer of the factory system in the textile industry, has not been the subject of a major biography before because of the paucity of documentary materials. Although note has been made by a number of scholars over the years of Arkwright's genius and role as an entrepreneur, the new material provides welcome background information on Arkwright's career while contributing to the history of the family dynasty.

Arkwright's spinning machines revolutionized the manufacture of cotton. He was founder of the modern factory system, creator of a system of production that was replicated by licensees and imitated by infringers; he created a prototype industrial system that even led his leading opponent to compare his achievements with those of Horatio Nelson and the Duke of Wellington. Arkwright, according to the first Robert Peel, was "a man who has done more honour to the country than any man I know, not excepting our great military characters" (p. 1). For the young man apprenticed to a barber in Lancashire, the career that led to the accumulation of a large estate, a knighthood, and portraits of his children painted by Joseph Wright, his upward social mobility was evidenced by material prosperity that, on his death, was said to have been "greater than that of most German principalities" (p. 219). His real and personal property was estimated at little short of £500,000 at his death in 1792.

The book, two-thirds of which covers the life of the first Richard Arkwright, contains chapters on the Arkwright family in Lancashire and his early life as a

barber, his partners and patents, the growth of his empire, and the patent trials leading, finally, to the annulment of his waterframe patent, paving the way for the imitation of the Arkwright system on a large scale in Britain. The great trial of *Rex v. Arkwright*, in which Arkwright's patents were overturned, was anxiously tracked by contemporary entrepreneurs. Mathew Boulton, Josiah Wedgwood, and James Watt, among others, were not a little concerned at the difficulty of patenting a system rather than an individual machine. Watt confided to Boulton, "Though I do not love Arkwright, I do not like the precedent of setting aside patents through default of specification" (p. 138). Not only was the question of the applicability of patents for a production system raised by the overthrow of Arkwright's patent but also questioned were the tactics of drawing up a specification. The concern revolved around whether it was preferable to specify with clarity or to risk the accusation that it was not possible to create the machine from the specification. Fitton claims that the summing up in Arkwright's case was, from the beginning, unfavorable (p. 135). Watt was more fortunate and the matter may have rested less in the difference between the respective patent specifications than in the remarkable skills of Watt's partner Boulton in parliamentary lobbying.

In a valuable chapter on the Arkwright system, Fitton takes the discussion of work organization in the cotton industry forward from Frances Collier's *The Family Economy of the Working Classes in the Cotton Industry* (1964) and S. D. Chapman's *The Early Factory Masters* (1967) with some richly descriptive passages on hours of work, child apprentices, diet, cleanliness in the factory, and factory discipline.

Arkwright's only son, Richard II, inherited the greater part of his father's fortune, including the entire cotton-spinning empire. The son had been a cotton spinner for all his working life, having learned his trade at his father's mills at Cromford. Within a year of his father's death, Richard sold a number of mills but retained the hub of his father's empire at Cromford and Masson. The younger Arkwright invested regularly in government funds, ultimately becoming the largest holder in England. He also laid out large sums on landed property, beginning with a small estate in Derbyshire in 1790, followed by large-scale investment in Derbyshire, Essex, Leicestershire, and Staffordshire. These investments placed him in the higher echelons of the landed gentry and the rents they brought in, added to the interest from the funds, provided the younger Arkwright with an increase in income, thus enabling him to accumulate yet further fortunes. Arkwright lent capital on mortgage to a number of members of the gentry and aristocracy, including a large loan to the sixth Earl of Chesterfield. Richard II moved into the stately home built, but never lived in, by his father in 1796 and sent three of his sons to Eton College. He became a benefactor of hospitals and religious organizations. By his will, Richard II left estates to each of his sons,

and £5,000 to each of his grandchildren (numbering over forty). Only one of the third generation of Arkwrights continued to be active in the cotton industry. Richard II's great wealth enabled his sons to enter the higher reaches of landed society and their estates were visible evidence of conspicuous consumption.

This meticulously researched book is a direct descendant of the Manchester tradition in economic and business history. The reader will search in vain for hypothesis, theoretical underpinning, and argument, but will be rewarded with the richness of description based on a meticulous lifetime's search for documentary materials on Arkwright. It is not a lively read but a product of solid scholarship.

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ADRIAN RANDALL. *Before the Luddites: Custom, Community, and Machinery in the English Woollen Industry, 1776–1809*. New York: Cambridge University Press. 1991. Pp. xvii, 318. \$59.50.

The Luddite disturbances of 1811–12 have traditionally been seen as the opening salvoes in the British labor movement's guerrilla warfare against the economic dislocation, worsening working conditions, and harsher social attitudes of the Industrial Revolution. Yet, given the increasing mechanization of the textile industries throughout the eighteenth century, it would be curious if resistance of this sort had not commenced earlier (and suspicions about the level of Luddite activity have been voiced by E. P. Thompson, John Rule, and other leading labor historians over the years). Adrian Randall's great merit is to ascertain that activism did begin earlier. Innovations on customary work habits and discipline were met with widespread protest and violence rooted in a moral economy hostile, as the older literature also insisted, to the newly popularized doctrines of laissez faire.

Randall's focus here is on the woollen industry in its two main concentrations, the west of England and the West Riding of Yorkshire. In each region work was differently organized, but in both places customary patterns, not only in the workplace but also in the community more broadly, proved resistant to the new machinery, whose impact, Randall argues, was considerably greater than previous historians have allowed. Offering general support for the work of Craig Calhoun and others, Randall defends the utility and analytic precision of the term "community" in both locales linking it to older regulatory legislation whose repeal in 1809 was bitterly disputed and arguing that the economic structure of each region did not alone determine its response to mechanization. Instead, existing traditions of protest, where violence was often carefully orchestrated rather than spontaneous, were crucial to patterns of resistance.

Sympathetic to his subjects, Randall does not dis-

miss their frequent plea that the factory system did not represent progress, but rather imposed a more stringent discipline and removed existing craft privileges. Moreover, he examines not only organized protest and riot but also the legal recourse to existing legislation, central to the reports of two Select Committees of the Commons in 1803 and 1806. These reports in particular reveal the "holistic moral economy" of regulation, stability, and customary control that the new confidence in laissez faire sought to supplant.

This is a powerful, compelling reexamination of a vital aspect of modern British industrialization in the great Thompsonian tradition; the chapter on "the political economy of machine breaking" is especially good. Randall sets a new standard for debates about the relation of custom to innovation in the period, and this book will doubtless be discussed widely.

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ROBERT A. STAFFORD. *Scientist of Empire: Sir Roderick Murchison, Scientific Exploration and Victorian Imperialism*. New York: Cambridge University Press. 1989. Pp. xii, 293. \$49.50.

The first decades of the Victorian era were banner years for British science, exploration, and imperialism, and in this short, densely written book, Robert A. Stafford seeks to show that the three were intimately related. Stafford also professes to the even more sweeping goal of examining the complex social matrix of Victorian science, including its economic, political, professional, public, and personal dimensions. Roderick Impey Murchison seems an ideal focus for such an ambitious case study.

Murchison was one of the most public of Victorian scientists. A world-renowned geologist, his position as Director-General of the Geological Survey of Great Britain gave him an official voice in government scientific policy. Less officially, as an officer or long-term council member of several London scientific societies, he was a prominent member of the small band of metropolitan scientists who shaped the course of much of British science during the middle decades of the century. Above all, Murchison was a consummate scientific politician who exploited his official and unofficial ties to the scientific, social, and political elite in Britain and abroad to pursue both personal and scientific goals.

Several recent studies have shown that as a geologist Murchison was both a gifted scientist and an ambitious scientific entrepreneur. In this book, Stafford shows that his interest and influence in geography were almost entirely as entrepreneur. Murchison's extensive travels in pursuit of his geological researches were confined to Europe, but he developed a world-wide network of informants, sup-

porters, and patrons. Thus, Stafford argues, as Murchison's interest in field work waned, he turned to geography and to scientific "statesmanship" as a way to extend the domain of his geological theories and enhance his prestige and influence at home. Stafford focuses especially on Murchison's activities as president of the Royal Geographical Society (RGS), arguing that under his direction the RGS became more rigorous scientifically, more popular with the public, and more useful to the government. As a forum for promoting public and scientific support for imperial expansion and official exploration, and as a major conduit for individuals seeking government support in private exploratory ventures, the RGS became a microcosm of the informal alliance between science, government, and finance, between public and private interests, that Stafford believes was an essential characteristic of the social matrix of Victorian science. It also gave Murchison a voice in almost every British expedition for more than thirty years.

Stafford has marshaled an impressive body of evidence to support his case, but his organization often subverts his ambitious synthetic aims. Six of the book's eight chapters, and usually several subchapters, consist of sequences of brief descriptions, arranged first by geographical region and then chronologically within each region, of the various exploratory expeditions in particular geographical areas. The result is a fragmented, sometimes repetitious presentation. When dealing directly with Murchison's activities, Stafford provides perceptive insights into the social and political dimensions of Victorian science, but by extrapolating too readily from Murchison's interests to those of "British science" and "British scientists," he claims a greater generality than his evidence will support. At times, too, Stafford's concern for geographic and chronological completeness diverts him from his principal argument and his descriptions become little more than an encyclopedic catalogue of who went where and why. Despite such shortcomings, if read carefully this book adds considerably to our understanding of Murchison and to our knowledge about the public role of science in the Victorian era.

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C. PETER WILLIAMS. *The Ideal of the Self-Governing Church: A Study in Victorian Missionary Strategy*. (Studies in Christian Mission, number 1.) New York: E. J. Brill. 1990. Pp. xv, 293.

If this book were entitled "The Victorian Debate about Multiculturalism," it would probably get the attention it deserves. Although addressed primarily to specialists in missionary studies, C. Peter Williams's book is also a fascinating, well-documented contribution to the study of Victorian thought about racial

difference, the nature of empire, and the social character of religion.

Until the 1890s the Church Missionary Society (CMS), the largest and most prestigious of the British missionary societies, propagated the ideas of the mid-Victorian missionary theorist Henry Venn, who had urged the missionary movement to foster the development of indigenous non-Western churches. Sensitive to the distinction between Christianity and civilization, and aware of the dangers of imposing English culture on non-Europeans, Venn wanted missionaries to promote "self-supporting, self-governing, and self-extending" churches outside the existing framework of the Church of England overseas. The goal of the missionary movement was, in Venn's well-known phrase, "the euthanasia of the mission."

Williams explains how Venn's thought evolved, how the CMS vigorously attempted to maintain Venn's policy in the 1870s and 1880s, and how his ideas were abandoned by the CMS by the 1890s. He also persuasively argues that the racial dimension of the controversy over church self-government, although never absent, was considerably more complicated than it has been portrayed. In 1864, the CMS had arranged for the appointment of a black Bishop in the Niger Delta, but on his death in 1891 he was replaced by a white Englishman. Some historians attribute this setback for progress toward an indigenous African church leadership to the emergence of a more virulent racism in the 1880s and 1890s. Williams sets this incident in the context of chronic conflict between the CMS bureaucracy in London and CMS missionaries in the field. Highly suspicious of missionary paternalism, Venn and his administrative successors suspected that CMS schemes for an indigenous church would run aground on entrenched missionary opposition. Missionaries in turn believed that remote London bureaucrats underestimated the problems involved in establishing an indigenous church. Williams takes Venn's side without question and consequently portrays the very diverse body of missionaries as uniformly paternalistic and power hungry. But he also demonstrates the dogged determination of the CMS bureaucracy to battle missionary paternalism.

By 1891, Williams argues, the CMS leadership had gradually fallen into the hands of inferior men, less aware of the dangers of paternalism and more concerned with bureaucratic peace. But Venn's Afro-philic ideas, still influential on the CMS home committee in 1891, also fell victim to the deeply entrenched suspicion of racism within the missionary movement. Venn had recognized that a multiracial church in a colonial setting would inevitably be dominated by the Europeans with superior academic training and financial resources. That is why he advocated autonomous, racially based churches, with overlapping separate episcopates for different races and cultures. Venn's critics in the 1890s argued that the doctrine of separate churches for separate races

was unacceptable for Christians committed to a universal, united, worldwide church. Missionary paternalists and apologists for episcopal power within the CMS successfully used Christian antiracism, just as they later used ecumenicism, to stifle movements toward church self-government.

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RONALD HYAM. *Empire and Sexuality: The British Experience*. (Studies in Imperialism.) New York: Manchester University Press; distributed by St. Martin's. 1990. Pp. ix, 234. \$59.95.

Perhaps the fastest growing and certainly one of the most popularized fields of British historiography is the study of Victorian sexuality. Ever since Stephen Marcus discovered the "other Victorians," historians have delighted in probing beneath the prescriptive codes of respectability and uncovering the realities of Victorian sexual behavior. Ronald Hyam applies the insights gained by this new scholarship to a study of the British imperial experience, and he not surprisingly finds extensive and diverse sexual contact between British men and colonized women and men. (He does not differentiate between homosexual and heterosexual behavior because he considers such categories ahistorical and culture-bound, even though the Victorians certainly made that differentiation.) Imperialism, he observes, was "not only a matter of 'Christianity and commerce,' it was also a matter of copulation and concubinage" (p. 2). Without monocausally attributing British imperialism to sexual lust, he argues that the empire offered Victorian men (he does not deal with Victorian women) the opportunity for sexual pleasures denied them in their own repressed society.

Other studies of British imperialism, such as Kenneth Ballhatchet's *Race, Sex and Class under the Raj* (1980), have examined the sexual relations between the rulers and the ruled, so Hyam's work does not offer a new understanding of the dynamics of imperialism. The strength of this work is in the great mass of examples Hyam has assembled of the diverse forms of sexual relationships and experiences in the empire. His glossary alone is enough to titillate the most jaded social historians. His explanatory comments, however, are often naïve and overgeneralized, and sometimes simply wrong.

Hyam, for example, avoids what he calls "pseudo-Freudian speculation," (p. 19) but, without considering psycho-sexual factors, his interpretations are often simplistic. Discussing British male lust for non-Western women and men, for example, he has trouble explaining whites' belief in aggressive black male sexuality. Although many historians who are not necessarily Freudian have insightfully drawn on the psychological concept of projection in explaining this myth, Hyam grounds his explanation in the supposi-

tion that the black man's penis is larger than that of the white man.

Hyam's work is also weakened by his narrow stereotyped rejection of feminist perspectives. Considering feminism "a conceptual framework which is itself . . . fundamentally hostile to sex," he argues that it is "the very last tool likely to be found useful for understanding the history of sexuality" (p. 17). This myopia seriously impoverishes Hyam's study; he completely neglects the experiences of both colonizing and colonized women. Viewing imperialism only from the white male perspective, he is able to conclude that sexual relations between the rulers and the ruled were not necessarily relations of dominance and exploitation, and even that they actually improved race relations. Although this provocative argument may have some merit, it ignores the powerlessness of the dominated and the realities of sexual violence.

A recurrent theme in Hyam's book is the glorification of what he, like his British forbears, sees as the carefree, polymorphically diverse sexuality of the non-Western world. Attributing tremendous invidious power to the late-Victorian purity movement, he claims that Westerners introduced the idea of sexual guilt to the colonies in the twentieth century. This ethnocentric view ignores indigenous forces favoring sexual control and purity and is especially suspect when he credits to it the puritanism of such movements as Maoism.

Despite its weaknesses, Hyam's book is a useful contribution to British imperial scholarship, providing much raw material for future work. If the information brought together by Hyam's extensive research could be analyzed by historians without the blinders limiting his understanding, then the sexual component of imperialism could truly be illuminated.

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PHILIPPA LEVINE. *Feminist Lives in Victorian England: Private Roles and Public Commitment*. Cambridge, Mass.: Basil Blackwell. 1990. Pp. xii, 241.

Victorian women were among the first to be rediscovered by feminist historians; the past twenty years has seen an outpouring of scholarship on virtually every aspect of their public and private lives by some of the best historians of our generation. Philippa Levine, building on the work of these scholars, offers a summary of the major feminist issues of Victorian England; politics, legal reform, education, work opportunities, and women's networks are all surveyed. As such, this book, like her earlier study, *Victorian Feminism, 1850-1900* (1987), is a useful introduction to the main actors and issues of the day.

Focusing on the lives of 194 well-documented feminists, Levine traces the numerous interconnections

among them; they mostly knew each other, were often related by marriage or education, and shared common goals that could overcome personal or ideological differences. Levine has an acute eye for the necessary pragmatism of an activist, combined with unexpected contradictions or idealisms. Emily Davies, for example, frustratingly cautious and rigid in her methods, refused to include radical women on the boards of her educational projects, but she never turned down their social invitations.

Levine rightly emphasizes the ways in which feminist thinking has been simplified or subsumed to contemporary ideological positions, so that we have created false unities or polarities between feminism and other movements. She is especially good on the class dimension of Victorian feminism, ably defending the efforts of these educated middle-class women to replace class criteria with those of gender. She points to how class was always a status conferred by male activity and how recalcitrant male trade unionists were about organizing women workers. Faced with an obdurate male definition of work, feminists had to set their own terms of debate here, as with so many other social issues. Rather than accepting a natural split between socialism and feminism, Levine contends that "in positing a solidarity premised on gender rather than on class, the feminist movement offered not conservative but subtle subversion of [trade unionism's] primary agenda" (p. 160).

The subversive undermining of society's most dearly held beliefs is an excellent summary of Levine's presentation of the private roles of feminists, for she demonstrates convincingly the ways in which they managed to remain pro-marriage while questioning every aspect of its legal, social, and religious foundations. She also provides a cogent summary of the ways in which feminists successfully questioned the inequality of the marriage contract but were unable to resolve their divisions over the labor contract. It would have been useful to have had more discussion of why late-nineteenth-century feminists disagreed so powerfully with each other over protective legislation.

I look forward to Levine writing a book-length study on feminism and class, but this is not it. She has moved into a crowded field and this book does not stand out. Too often her conclusions are familiar summaries of others' work. After demonstrating the ability of Victorian feminists to question all the assumptions of their day, she fails to address the problem of the "separate spheres ideology." Too often it appears to be a monolith that feminists could only subvert but never confront. Although much personal information about individual feminists is related to the major issues, the result lacks the depth of a more focused study. The decision to concentrate on the lives of nearly 200 certifiable feminists gives the book texture and resonance for anyone familiar with the period (I welcome the skillful use of letters to and from friends). Unfortunately, however, Levine

did not include a list of these women or their dates of birth and death; since they form the basis of her study, the book cries out for such minimal documentation.

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IAN NEWBOULD. *Whiggery and Reform, 1830-41: The Politics of Government*. Stanford: Stanford University Press. 1990. Pp. x, 401. \$45.00.

Whigs, like their successors in the Liberal Party, continue to fascinate historians. Is it the loss of parties of moderation that attracts or a morbid fascination with the long dead and slowly dying that explains this occurrence? Ian Newbould's new book gives no clue, although it does illustrate that well-tilled fields can yield something of substance.

Newbould's contribution to the Whig canon is a thorough narrative of political decisions and strategies for legislation during a seminal decade of reform. Using more than two dozen manuscript collections and as many newspapers and journals, Newbould sets out to prove that the Whigs were single-minded, independent adherents of "moderate reform, timely conceded" (pp. 9, 314), and that they did not merely succumb to Radical pressures as some historians have claimed. Nor did Whigs necessarily legislate out of fear of revolution; Lords Durham and Althorp saw reform as a good in itself, if Earl Grey and Viscount Melbourne did not. Whig reform was designed to uphold rank, property, and authority; extending the suffrage to a deferential and conservative middle class could only redound to Whig advantage. If none of this is startling, it is useful to be reminded how conservative the Whigs were and how much of the Reform Bill of 1832 was to protect, not to alter, the status quo.

A secondary theme in Newbould's book is the state of political parties. In contrast to those who have seen firm party alignments by the 1830s, Newbould stresses the fluid arrangement of political groups in the House of Commons. Indeed, an essential support for the Whigs after 1832 was given by the moderate conservative, Sir Robert Peel. Thus, a Whig-Peelite coalition, acting as a centrist majority, stood against the extremes of both Radical and ultra-Tory. The significance of Newbould's claims, however, is sometimes obscure. For clarification, one should turn to Angus Hawkins's treatment of party and parliamentary government ("Parliamentary Government and Victorian Political Parties, c. 1830-c. 1880," *English Historical Review* 104 [July 1989], 638-69).

Overall, this is a book worth reading for its deep knowledge of the Whig elite within a limited parliamentary context. Yet one wonders whether the impressive reforms of that decade—including the reform of the franchise, factories, the Poor Law, and

municipal corporations—can be seen so exclusively as the work of a centrist-conservative coalition. Surely there is an important role (here barely mentioned) for philosophical radicals such as James Mill and Jeremy Bentham as well as for Tory humanitarians such as Michael Sadler, Richard Oastler, and Lord Ashley. Peter Mandler's book, *Aristocratic Government in the Age of Reform: Whigs and Liberals, 1830–1852* (1990), is a necessary corrective. Mandler does not neglect the role of popular movements or the impact of the “machinery of inquiry” in defining the reform debates and in shaping public opinion. Thus, he bridges the gap between high and low politics left wide by Newbould.

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DAVID W. GUTZKE. *Protecting the Pub: Brewers and Publicans against Temperance*. (Royal Historical Society Studies in History, number 58.) Wolfeboro, N.H.: Boydell, for the Royal Historical Society, London. 1989. Pp. 266. \$73.00.

This is a useful monograph on a familiar late-Victorian and Edwardian topic, the political influence of the drink trade. David W. Gutzke presents a well-controlled and careful, if somewhat cautious, examination of the pressure-group tactics and the organizational problems of this vested interest in its struggle against threatened confiscation, regulation, and taxation. The central theme that emerges from Gutzke's investigation is the fragility of any national alliance over time, involving as it did striking regional and sectional differences—particularly the inherent rivalry between brewers and retailers—and long traditions of autonomy. In spite of the great resources of some of the brewers, the organizations that attempted to mount resistance to state interference were not lavishly endowed. Consequently the story is one of modest success, especially in the 1890s when the trade was prospering and the opposition was divided, but not of the enormous influence darkly assigned to the drink trade by Liberal politicians. In this, Gutzke's interpretation is somewhere between the older historiography and recent revisionism.

Gutzke does not ignore the larger issues of cultural and economic context: for example, the way changes in both middle and working-class drinking habits affected the play of the temperance issue or how the spread of shareholding in the larger breweries deepened the trade interest. Nevertheless this study does not often rise above a well-defined institutional focus. It is a solid dissertation turned into a monograph and as such has found an appropriate home in the nicely produced Royal Historical Society series (with footnotes where they belong, at the foot of the page). Aside from the no doubt unfair complaint that this is not a different kind of book, one criticism might be

offered. Gutzke is careful to engage recent historiography in a dialogue within his text, but he does not directly discuss the seminal work of Basil Crapster, whose unpublished dissertation of 1949 has long been recognized as the important pioneering study of the political activity of the drink trade (the work is at least cited in Gutzke's bibliography). Gutzke has clearly gone back to the original sources, to trade newspapers, minute books, annual reports, and society correspondence, but one would have expected him to indicate explicitly what debt he might owe to his predecessor and exactly where, why, and how he has moved beyond Crapster's interpretations.

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ROB SINDALL. *Street Violence in the Nineteenth Century: Media Panic or Real Danger?* New York: Leicester University Press; distributed by Columbia University Press. 1990. Pp. 168. \$39.00.

Despite the title, Rob Sindall's book is less a study of street violence than an attempt to prove that Stanley Cohen's model of moral panic applies to Victorian England. In fact, Sindall's final conclusion is that Cohen's sociological model can be applied over time—a rather limited conclusion for a work of history, especially since Jennifer Davis has already made the case quite convincingly (“The London Garrotting Panic of 1862: A Moral Panic and the Creation of a Criminal Class in Mid-Victorian England,” in *Crime and the Law: The Social History of Crime in Western Europe since 1500*, edited by V. A. C. Gatrell *et al.* [1980]).

Historians of crime and justice face particular problems in terms of definition and perception. Official statistics are suspect on a number of grounds, but literary evidence may be even less reliable. Sindall attempts to address this problem by suggesting that criminal statistics be used to assess the middle-class perception of crime rather than its reality. Although this idea is a sound one, Sindall fails to use it consistently. In evaluating press reports, he often contrasts the sensationalism of press accounts with the factual accuracy of the statistics. According to his thesis, however, the contrast is actually between two different perceptions of an unknowable reality.

Sindall assumes that concern about crime is a product of middle-class perceptions that are “at one and the same time moulded and reflected by the press” (p. 131). He looks to the editorial pages of the major London newspapers as his major source for public opinion. “It was in the letter columns and the editorials that the opinions of the ruling elite could be expressed, exchanged and changed” (p. 131). Sindall finds the press guilty of sensationalism and even suggests a fraud on the public referring to “the many instances in which the middle classes were the unwitting pawns in the hands of a not so witless press in the

process of manufacturing a crime wave" (p. 82). But his own evidence shows that the press was neither monolithic nor consistent in its reporting of and editorializing about crime. Certainly there were cases in which newspapers created panics based on a few isolated attacks, but there were also instances in which editorials warned against overreaction.

Instead of forcing his evidence into Cohen's model and berating the press for deception and sensationalism, Sindall could have devoted more attention to a critical analysis of the meaning of street violence for the Victorian middle class. He spends a great deal of time discussing secondary works and disappointingly little time examining actual incidents of street violence and the contemporary accounts of them. As a result, Sindall offers surprisingly few conclusions about either street crime or middle-class perceptions of it.

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W. J. FORSYTHE. *Penal Discipline, Reformatory Projects and the English Prison Commission 1895-1939*. Exeter: University of Exeter Press. 1990. Pp. 255. £25.

In this book, W. J. Forsythe carries on the account of modern British penal policy he began in *The Reform of Prisoners 1830-1900* (1987). Whereas the former work covered an era well-trod by scholars, this sequel deals with a period (particularly after 1914) that has not yet, other than in regard to juvenile offenders, been examined in depth. The book thus offers much fresh detail, often drawn from recently opened official archives.

This detail is marshaled in support of a clear and consistent point of view. As in his previous book, Forsythe is concerned to restore the reputation of penal reformers after the harsh treatment they received in recent years from revisionists inspired by Michel Foucault. In that work his criticism was directed primarily at Foucault and Michael Ignatieff. Here, however, he takes issue with David Garland, whose *Punishment and Welfare* (1985) developed a Foucauldian interpretation of the reforms of 1895-1914. Forsythe argues that rather than expressing a drive toward the creation of a more extensive and coercive disciplinary system, penal reforms proceeded from genuinely humanitarian impulses and were constrained throughout by an intellectual and legal framework that was both moralistic and libertarian. In England, at least, the state remained less total and more classically liberal than in the Foucauldian scheme of things.

If he defends the aims of penal reformers and administrators, Forsythe also is at pains to point out the wide gap between their professions and their practice, between the inspirational rhetoric of rehabilitation and the depressing everyday life of prison-

ers. If he sees antiutopian visions of a totalitarian "penal-welfare complex" as a caricature of reality, he finds equally illusory utopian vistas of a caring state restoring its wayward members to full membership in the community. The real progress he locates in this period lies more in the diminution of prison populations and the amelioration of prison conditions than in genuine reformation and restoration of criminal offenders.

Forsythe concludes by drawing from his subject some chastening lessons for future penal reformers. He finds that without genuine support from bureaucrats, courts, and public opinion, reforms are unlikely to be effective and that reformers are best advised to scale down expectations of dramatic change.

Useful and sane as it is, this book never questions the term "reform" itself, nor does it escape a certain narrow preoccupation with the immediate tasks of penal administration. It eschews the deeper questions whose raising made Garland's work, even when it did not compel assent, so stimulating, and which my own book, *Reconstructing the Criminal* (1990), has addressed. How does society decide what sort of treatment is appropriate for different sorts of behavior and different sorts of persons? Why do these decisions change? How and why does the meaning of "reform" change from generation to generation? What relation does penal policy bear to other sorts of governmental policies? Such questions ought to be at least acknowledged in any history of penal policy; historians and sociologists of punishment should continue to read Garland alongside Forsythe.

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CHRISTOPHER HAMLIN. *A Science of Impurity: Water Analysis in Nineteenth-Century Britain*. Berkeley and Los Angeles: University of California Press. 1990. Pp. xiii, 342. \$39.95.

What makes water pure or impure? How can one know? The answers of scientists who scrutinized the water supply of nineteenth-century London provide Christopher Hamlin a vehicle for exploring the ways scientific expertise became a prominent part of public decision making. During the nineteenth century, science came to be enshrined as a neutral means of resolving conflict. Yet Hamlin convincingly shows that at every turn, the social and political programs of water analysts fundamentally shaped their interpretations of evidence. Scientific discovery did not determine water standards, he concludes, for experts speaking for private water companies or other political interests pressed scientific arguments into the service of the particular conclusions they wished to sustain.

Hamlin casts his rich narrative chiefly in two contexts: the participation of scientists in policy making, and changing practices in science. He traces the

origins of water analysis to chemical studies of mineral waters. He then explores the occasions when concern about London's water supply became acute (particularly during cholera epidemics), shifting methods of assessing potable water, the social and political commitments of water scientists, and the ways scientific pronouncements were translated into governmental policy.

In a deft stroke, Hamlin shows that while there was a gradual shift from chemical to bacteriological criteria of purity, the advent of bacteriology neither clarified definitions of danger nor disentangled water analysis from political disputes. Pre-bacteriological microscopists could only comment on the aesthetics of polluted water, pointing out that the animalcules populating the Thames were "larger, fatter, and uglier" (p. 110) than those found elsewhere, whereas by the 1880s bacteriologists could isolate the specific causative agents of cholera and typhoid fever. The new science did not give clear guides to action; as its authority increased, however, it did provide confidence.

Over time, moreover, the nature of expertise changed. Unlike earlier experts, who confidently asserted possession of a simple truth, late-nineteenth-century water analysts celebrated their mastery of a complex literature and esoteric techniques. Competition among the various water factions abated in 1904 with public ownership of the water supply. But the symbolic authority that scientific expertise commanded endured, as did a mode of expertise that had mystification as one of its hallmarks.

Hamlin has a fine sense of the theatricality of expert testimony and skillfully delineates the strategies analysts employed to make their authority compelling to policy makers. How this drama played to its wider audience remains missing from Hamlin's account. We learn little about how Londoners regarded the water they drank or the judgments of experts, yet such lay perception is one key element in the construction of expertise and in the cultural authority granted or withheld from science.

This study represents the best standards of recent work in the history of science and public health. Hamlin steadfastly declines to see social action as determined by scientific progress. Still, he recognizes that although the idiom of science was flexible, it was not arbitrary; to understand scientific polemics, historians must seriously engage with the changing technical content of scientific thought and practice. By embracing complexity, he has produced an important account of the social construction of expertise and its place in modern society.

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DAVID CANNADINE. *The Decline and Fall of the British Aristocracy*. New Haven: Yale University Press. 1990. Pp. xiv, 813. \$35.00.

In the last quarter of this century, the focus of social history in and of Britain has shifted from the poor and underprivileged to the rich and overprivileged, from the retrospective radicalism of E. P. Thompson, Eric Hobsbawm, and John Saville to the tempered nostalgia of Lawrence Stone, John Beckett, and J. C. D. Clark. Yet David Cannadine's book is the first and certainly the most comprehensive attempt to explain in depth the astonishing eclipse of the landed elite as a whole in the last one hundred years. It is a blockbuster of a book, brilliantly erudite and wittily provocative. It traces the steps by which the landed aristocracy, who dominated British society, politics, and, to a large extent, the economy for centuries and were at their zenith of wealth and power from the Glorious Revolution to the 1870s, gradually lost their hegemony to the plutocracy and democratic politics of the twentieth century.

In 1880 the landed class still united in themselves most of the wealth, status, and power of the United Kingdom, the worldwide superpower at the head of the largest empire that the world had ever seen. They owned more of its territory than any other ruling class. Approximately eleven thousand families owned two-thirds of the British Isles, including three-quarters of Ireland and nine-tenths of Scotland. The landed elite filled the majority of the House of Lords, three-quarters of the House of Commons, a majority of the cabinet, all of the offices of the royal household, and most of the county offices from lord lieutenant to justice of the peace, and they controlled recruitment to the civil service, the armed forces, the bishops' bench, and the judiciary. In the truest sense, the landed class was the governing class, a single unified elite.

How, in a short one hundred years, did they come to suffer such an ignominious and humiliating decline? It began with what I have called the "earthquake of the 1880s" (*The Origins of Modern English Society* [1969], 454), the geological shift in the structure of politics that alienated the landlords from the Liberal Party under the impact of the Irish Home Rule crisis, the democratic franchise of 1884, and the threat of social and land reform and left them vulnerable to despoliation whenever "middle-class monsters" such as Lloyd George or Neville Chamberlain came to power. The decline was accelerated by economic factors, the agricultural depression, and the steep fall in rentals and land values, soon overtaken by the vast fortunes of the new industrial, financial, and colonial millionaires. And it was exacerbated by the self-destructive behavior of the elites, their frustration of democratic legislation that provoked the clipping of the powers of the Lords in 1911, their near-treasonable opposition to Irish Home Rule in 1911-14, and their decision, in F. M. L. Thompson's phrase, to "liquidate the landed interest" by the land sales of 1910-22, the largest transfer of property since the Reformation or even the Norman Conquest.

Much of this is well known from the work of Michael Thompson, W. D. Rubinstein, myself, and others, although here it is detailed at greater length. Cannadine has added a penetrating survey in the second half of the book of the reactions of the decayed patricians and their offspring to their waning glory that reconciles a range of otherwise inexplicable and bizarre behaviors: the effete and ineffectual politics of the "Hotel Cecil" and their kind; the fascism of Sir Oswald Mosley, Lord Garvagh, Lord Ernest Hamilton, Unity Mitford and Diana Mitford (Lady Mosley); the equal and opposite reaction of Nancy Mitford and Jessica Mitford, who went as far left as their sisters went right, along with John Strachey, Christopher Isherwood, and other "public school lefties"; the colonial escapism of lords Erroll and Delamere and the hedonists of Kenya's Happy Valley; and the greedy and unbusinesslike peers such as lords Albemarle, Winchilsea, and De la Warr, who were gulled by company-promoting charlatans.

Not all of the patricians were failures, and Cannadine documents the success of dukes such as Westminster, still one of the richest men in the world, and Bedford, who like many others has turned his country seat into a museum and theme park, and of highly successful businessmen such as lords Derby and Harlech, television tycoons. In my computer study of elites, the descendants of the great landowners of 1880 were still the richest of all the elites in the 1960s and 1970s, second only to the overlapping millionaires (*The Rise of Professional Society* [1989], 262–63). These, Cannadine argues, were simply the lucky or cleverest survivors now merged with the super rich. A short notice cannot do justice to this splendid book, which should be required reading for every historian of British society and politics.

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FRANS COETZEE. *For Party or Country: Nationalism and the Dilemmas of Popular Conservatism in Edwardian England*. New York: Oxford University Press. 1990. Pp. x, 221. \$39.95.

It is customary to think of factions or pressure groups, with accompanying acronyms, as the peculiar province of leftist regimes and politics. Frans Coetzee, however, convincingly dispels this assumption for Edwardian England by showing how the Conservative Party was influenced by an assortment of interests committed to a nationalist agitation. The politics of deference, which had served nineteenth-century Conservatives so well, no longer appeared able to address the defense, economic, and social challenges of the new century. A more forthright appeal to the increasing electorate seemed necessary; but, as Coetzee asks, was it to be framed along party or national lines?

This question is explored mainly through three emergent pressure groups: the British Navy League, the Tariff Reform League, and the Anti-Socialist Union. Distinguished by their spunk and singleness of purpose, these associations had a broad appeal to working people, but they caused no small degree of frustration to party leaders. That their rise coincided with an unprecedented hardening of party lines only exacerbated this internal conflict. The pressure groups tested what Coetzee calls "the permeability of partisan barriers," which in practice "meant determining what should be done with nationalists who were not Unionists, or even Unionists who were deemed to be not sufficiently nationalist" (p. 59). It is hardly surprising in the wake of the electoral cataclysm in 1906 and the ensuing spate of Liberal legislative measures that nationalists should have sought refuge with the Conservatives. While Liberal governments under Henry Campbell-Bannerman and Herbert Asquith were increasingly viewed as soft on defense, suspect on preserving private property, and impossible on the issue of protection, the Conservative Party portrayed itself as a haven for "English virtues and national interests" (p. 123). Tariff reformers even threatened, at least until 1913, to drag the party into an abandonment of Britain's classic free trade stance. The "Socialist menace" and an ill-defined pressure from the Labour Left on Liberal policies also no doubt drove wary nationalists into the Conservative camp. By 1911 these partisan tendencies were further magnified when Home Rule came to occupy the center of the political stage. Eventually, however, with the party truce during the war, the influence of pressure groups became negligible because of declining memberships and funds. Their legacy can be traced to such patriotic organizations as the British Commonwealth Union and the National Party and even to Stanley Baldwin's electoral triumph in the 1920s.

Despite the turmoil attendant on their two decades of vitality, Coetzee concludes that nationalist pressure groups energized the "radical right" and their "collective discontent signaled a 'crisis of Conservatism'" (p. 163). Notable in its omission is any discussion of pressure groups spawned by the Irish issue, such as the Irish Unionist Alliance. Also, there is a tendency to underestimate the potency of such traditional issues as the franchise, disestablishment, and Home Rule and to focus unduly on class as "the primary issue in political mobilization" (p. 63). Otherwise, there is much to admire and savor in this exceptionally thorough assessment of Edwardian pressure group politics. At the very least, it helps us to understand how Conservatives have acquired the epithet of "the national party."

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ALFRED GOLLIN. *The Impact of Air Power on the British People and Their Government, 1909–14*. Stanford: Stanford University Press. 1989. Pp. xii, 354. \$39.50.

In this meticulously researched and documented second volume of a projected multivolume history of British air power, Alfred Gollin illuminates a formative era in air history. He skillfully interweaves military, diplomatic, social, political, and technological history into a detailed and fascinating account of the early years of air power in Britain. Gollin shows the effects of aeronautical developments from the beginning of official British exploration of the question of military aeronautics to the outbreak of war in 1914 and analyzes the reasons for Britain's failure during this period adequately to address air power's potential role in war.

Arguing that interservice rivalry largely accounted for this neglect, he underlines "the absolute failure" (p. 203) of the War Office and the Admiralty to cooperate in home air defense. The War Office's insistence on its sole responsibility for home air defense while focusing its aeronautical efforts instead on reconnaissance prevented the creation not only of a home air defense system but also of a unified air service.

Gollin is particularly adept at presenting the personalities who played a significant role in both advancing and thwarting the development of air power in Britain. Among the former, he portrays Secretary of State for War Richard Burdon Haldane as single-minded in his pursuit of a scientific foundation for an efficient air service, while First Lord of the Admiralty Winston Churchill emerges as the savior of British air power.

Tracing the emergence of air power politics in Britain, Gollin emphasizes three key events: the House of Commons debate in August 1909 on Britain's weakness in the air; the severing of formal ties between the enthusiasts of the Aerial League of the British Empire and the government in March 1910; and the inept presentation of army estimates to the House of Commons by Colonel J. E. B. Seely, Under-Secretary of State for War, in March 1913. He presents the crucial role played by public opinion and pressure groups in the politicization of the air power question.

Gollin also shows the impact of developments in the international arena, particularly the growing Anglo-German rivalry, in awakening the government to the threat that air power posed to Britain's traditional defense policy. German behavior in the first International Conference on Aerial Navigation in May 1910 and the 1911 Agadir crisis led to closer attention to the problems of air defense. Nevertheless, a major change in Britain's defensive arrangements did not take place until September 3, 1914, when the War Office relinquished to the Admiralty responsibility for the aerial defense of Britain. The volume concludes with Churchill's assumption of control of

home air defenses, which Gollin views as marking the transition from one era of British air history to the next.

With its thorough examination of the air power debate in Britain, this book makes an important contribution to our understanding not only of British strategic thought but also of British politics and society on the eve of World War I.

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STEPHEN R. WARD. *James Ramsay MacDonald: Low Born among the High Brows*. (University of Cincinnati Studies in Historical and Contemporary Europe, number 3.) New York: Peter Lang. 1990. Pp. xvi, 341, \$59.95.

No historian likes to throw away research material, but that is what the author of this book should have done. Stephen R. Ward appears to have begun his work a year or so before the publication of David Marquand's *Ramsay MacDonald* (1977). Ward nevertheless continued, fully aware that Marquand had set the standard by which all other biographies of MacDonald would be measured. Yet, quite apart from that weighty consideration, Ward's is a clumsily written and poorly edited account (for examples of the awkwardness of this book's prose, see pp. xii, xiii, 14, 17, 76, 88, 90, 92, 114, 128).

Two claims are made for the originality of the present work: that it offers a more critical insight into MacDonald's complex personality; and that it provides a more critical assessment of MacDonald's 1914 decision to oppose British entry into the war. Neither claim is justified.

The quality of the author's argument about the first claim is neatly indicated by his subtitle. Certainly no British prime minister started life with a greater social handicap than MacDonald, but Ward makes no sustained attempt to capture the effect of his being the illegitimate son of a Scottish peasant woman. Psychological analysis is not the author's strong point; he writes, indeed, that MacDonald's "behavior is easier to describe than evaluate" (p. 285). Ward is content, for instance, to note that as a child MacDonald "modified his personality to . . . protect his sensibilities" (p. 282). Similarly, after mentioning that MacDonald's wife "was easy to talk to and understood his faults," Ward concludes: "When she died, MacDonald found that outlet much diminished" (p. 26). As for the second claim, it amounts to little more than a feeble assertion: "While MacDonald never confessed that he was acting furtively, there are several indications that it may have been the case" (p. 83).

This book adds virtually nothing of substance to Marquand's masterly portrait.

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CHRIS WRIGLEY. *Lloyd George and the Challenge of Labour: The Post-War Coalition, 1918–1922*. New York: St. Martin's. 1990. Pp. viii, 326. \$39.95.

The armistice of November 11, 1918, did not bring "normalcy" to Great Britain. Not until 1922 did tranquility return to the British Isles. Between 1918 and 1922, David Lloyd George's coalition government grappled with revolt in Ireland, unrest in the armed forces and the police, Bolshevik agitation, industrial strife accompanied by a trade union challenge to the government, inflation, and economic dislocation caused by the transition from war to peace. The propertied classes were gripped with genuine, though unfounded, fear that London would follow on the heels of Petrograd, Berlin, and Budapest in experiencing Marxist upheaval. Lloyd George, "the man who won the war," faced the prospect of losing the social and economic peace on the home front.

Chris Wrigley has written a solid study of Lloyd George's leadership of the postwar coalition that focuses on his increasing difficulties with the Labour Party. This volume serves as a sequel to his well-received *David Lloyd George and the British Labour Movement* (1976). When the Conservatives rejected him in 1922, Lloyd George, long alienated from the Asquithian Liberals, had also lost his radical credentials in the eyes of most sectors of the Labour Party. One of Britain's greatest prime ministers, great in large part because of his enormous gifts as a political tactician, was finished as a major force in British politics.

Lloyd George was a victim of both circumstances and his own methods. The surprise, really, was not that his coalition collapsed on October 19, 1922, but that it survived as long as it did. At the war's end, he genuinely wanted to create a "land fit for heroes." Initially he emphasized social reconstruction, partly because of a sincere commitment to social justice, but also because of political considerations. He characterized social reform as "cheap insurance" against Bolshevism (p. 82). He also hoped to create a national party through his domination of the middle ground of British politics. The "wizard of Wales," however, soon ran afoul of two competing interests: Liberal economic orthodoxy, which demanded a deflationary governmental policy; and the challenge of Labour, especially the potent strike threat by the Triple Alliance of miners, railwaymen, and transport workers, and leftist sentiment for nationalization. Meanwhile, the Right remained apprehensive that the streets might explode in class fury at any moment.

In the socioeconomic realm, whether it was a question of nationalization, the threat by strikers to undermine the government's constitutional position, or Britain's international competitive position, Lloyd George ultimately found himself at odds with Labour. Yet his muscular response to the challenge of the Left, which included strikebreaking, red-baiting,

and anti-Labour propaganda, did not win over a majority of the Conservatives to a new party. Wrigley also emphasizes Lloyd George's predilection for the grand gesture, his "live now, pay later" political style (p. 169), and his legendary cunning. His negotiations often raised expectations yet resulted only in the appearance of a solution.

Wrigley's treatment is supported by extensive research, including newspapers, journal articles, government records, and the papers of over fifty individuals. Every research library should have this important addition to the literature on Lloyd George and to the historiography of postwar Britain's unstable economic and social situation.

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W. R. GARSIDE. *British Unemployment, 1919–1939: A Study in Public Policy*. New York: Cambridge University Press. 1990. Pp. xvi, 414. \$69.50.

The failure of Keynesian economics to solve the problems of persistent unemployment and inflation during the last two decades has led many economists to question the validity of both Keynesian economic theory and policy prescriptions. The academic and political success of this "new" classical economics, variously associated with the advocacy of monetarism and microeconomic policies on both sides of the Atlantic, has led economic historians to reexamine British interwar economic policy from these conservative perspectives. Some have argued that even if a dramatic interventionist plan, such as that offered by David Lloyd George, had been implemented, it would have only reduced unemployment by one-fifth. Others have argued that high unemployment in interwar Britain—except for the cyclical crisis of 1921–22 and the worst of the world slump in 1931–32—was essentially regional and structural; and no reasonable amount of Keynesian stimulation would have alleviated the problem. Some have even argued that the level of unemployment insurance and relief payments made a good deal of the unemployment of the period voluntary. Unfortunately, many of these critical issues are discussed in a widely scattered journal literature that is often highly technical, econometric, and sometimes counterfactual. What W. R. Garside has accomplished in this admirable study is to provide a readable and comprehensive guide to the literature on British interwar unemployment. The author, to be sure, also takes sides. He offers a persuasive defense of Keynesian economics, economic policy, and economic history.

Garside's substantial study is well-organized and clearly written. Although it contains many statistical tables and a sophisticated economic analysis, it is largely free from the jargon that often disfigures so much of contemporary economic history. After defining the nature of the unemployment problem, he

leads us competently through a discussion of the unemployment insurance and "dole" payments of the period. The third part of the book examines the impact of international issues such as the return to gold, the abandonment of gold, tariff policy, and imperial schemes for relieving unemployment. The fourth part is the most original and valuable, for it brings together recent research that emphasizes the structural and regional nature of much of the unemployment problem between the wars. In the final part Garside turns to macroeconomic issues. He argues that by 1930 a majority of British economists, as well as some political figures, were urging the government to adopt a radical interventionist fiscal policy. Garside admits that the schemes offered would not have solved the whole problem, but they could have solved at least part of the problem, particularly if they had included efforts, suggested at the time, to deal with structural problems. Of little consequence, he wisely notes, was the fact that Keynesian theory was not yet fully available to justify theoretically a radical interventionist policy. It was not so much orthodox theory but custom, tradition, and a profoundly conservative hope that capitalism must be given a chance to right itself that prevented industry, labor, and government from attempting to solve the problem through vigorous state intervention.

This study is not a pathbreaking new interpretation. Much of the material will be familiar to specialists. This is, however, the best available comprehensive guide to the crucial problem of unemployment and public policy during the period. Garside's study can be combined with Peter Clarke's study, *The Keynesian Revolution in the Making, 1924–36* (1988), to provide us with a much better understanding of the unemployment problem between the wars and the revolution in economic thought and policy that it helped spawn.

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JONATHAN SCHNEER. *George Lansbury*. (Lives of the Left.) New York: Manchester University Press; distributed by St. Martin's. 1990. Pp. ix, 230. \$39.95.

"A project for the ages" (p. 123) is how Jonathan Schneer ironically describes George Lansbury's heroic if futile efforts to recast the British Left, first undertaken in 1889 when Lansbury abandoned his radical Liberalism and joined the Social Democratic Federation. For a half-century thereafter, his impressive figure graced many a platform of the British Left and his Christian and socialist beliefs led him enthusiastically to support many causes advocated in those precincts. Late in his life, he served as Leader of a Labour Party riven by internal division and confronted with external menace; indeed, he reluctantly

yielded that position rather than abandon his deeply rooted pacifism. Lansbury remains a revered figure in the party's history and, more widely, in the disparate ranks of the Left in Britain, which justifies his inclusion in the ongoing biographical series, "Lives of the Left."

His contributions to the cause and his role in British politics have been on the whole fairly assessed in this brief, thematically organized biographical treatment, which deliberately sacrifices comprehensiveness to depict Lansbury's consistency of vision and purpose as socialist, feminist, and pacifist. The theme is worthy, but its selectivity causes an imbalance in Schneer's depiction of Lansbury's political role. Furthermore, by pursuing the chronological evolution of Lansbury's views within each of these commitments, Schneer sacrifices overall chronological treatment. For example, Lansbury's important activities as organizer of the post-World War I revolt of local government ("Popularism," which echoed into the Thatcher era) precede a treatment of his involvement with Emmeline Pankhurst and Christabel Pankhurst and the suffragettes in the prewar years. Certainly, whatever the strengths of the present volume, this study in no way replaces the biography published in 1951 by Lansbury's son-in-law Raymond Postgate, which still ranks among the best treatments of Labour's early leaders.

Yet these costs are adequately compensated for by Schneer's careful evocation of what he describes as his subject's "enduring alternative vision of socialism which Labour's leaders have never been able to appreciate—nor yet dispatch" (p. 5). Thus, Lansbury is securely placed in the Left's millenarian tradition, denied the realization of his goals by the compromising forces of "Labourism." Within the Labour Party, the conflict can be depicted in terms of the rival designs for Labour to function as a "party of government" or as a more principled "party of opposition." It is safe to say that no Leader of the Labour Party has ever been more identified with the latter label than Lansbury. Schneer also convincingly portrays Lansbury as an able politician, for the most part, and a skilled organizer. There is a greater degree of complexity in the political leader, if not the man, than historians have recognized.

In Schneer's discussion of the mutually reinforcing themes of Lansbury's socialism, feminism, and pacifism, the second breaks new ground both in terms of the contributions of subject and author. Lansbury moved beyond advocacy of extending the woman's "sphere" into the political realm to argue the need to abolish the very notion of "separate spheres." His treatment of Lansbury's pacifism yields little that is new; what is interesting, however, is that such a sympathetic biographer parts company with his subject's advocacy of pacifism. He criticizes Lansbury's stubbornness and "tunnel vision" as of 1937 (others would locate it in 1935 or even earlier), which led to an unsavory meeting with Hitler and revealed a view

of Europe "hopelessly outdated" (p. 186). Yet historians less sympathetically disposed to the animating force of Lansbury's pacifism could acknowledge the rigor and consistency of his views, which would brook no compromise for whatever reason. Nonetheless, it is a measure of Lansbury's flawed political judgment that his biographer at the very last refuses to accept Lansbury's moral lead. In the course of Lansbury's public career, many others, whatever their enthusiasm for this attractive figure, discovered good reason to refuse to accompany him the full distance of his millenarian's pilgrimage in a world that was not about to be inherited by the meek.

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CHRIS WRIGLEY. *Arthur Henderson*. (Political Portraits.) Cardiff, Wales: GPC; distributed by Books International, McLean, Va. 1990. Pp. xii, 210. \$26.00.

Arthur Henderson was a major figure in the development of the Labour Party in the first half of the twentieth century. The architect, together with Sidney Webb, of the constitutional changes that transformed the strictly working-class faction into a broadly based national party after 1918, foreign secretary in the second Labour government, and the leader who helped rally demoralized Labourites after Ramsay MacDonald's defection in 1931, he has nonetheless lacked adequate biographical treatment until recently. Mary Agnes Hamilton's *Arthur Henderson* (1938), published soon after his death, is now useful chiefly as a primary source. In 1989, however, F. M. Leventhal published a competent brief biography, which is now complemented by Chris Wrigley's equally succinct volume.

In the absence of any collection of Henderson's papers, Wrigley has based his study on a wide range of archival sources including trade union records, Cabinet documents, and the diaries and correspondence of his associates. The picture that emerges, while somewhat fuller on the prewar years than that of previous works, contains few surprises. One of the many Labour Party leaders molded by trade unionism and Methodism, Henderson exemplified the characteristic virtues of that worthy group. Solid, "relentlessly moderate" (p. 40), and selflessly devoted to the movement in which he spent his life, he was remarkable only for his extraordinary organizational ability, which contributed mightily to the growth of the Labour Party in the 1920s and to its revival after the electoral debacle of 1931.

Despite his lack of magnetism or oratorical skills, Henderson proved to be a competent foreign secretary, working successfully to strengthen the arbitration machinery of the League of Nations. To be sure, Adolf Hitler's accession to power soon made such

achievements pathetically irrelevant, and the later failure of the Disarmament Conference, of which Henderson served as chairman, was further evidence of the inadequacies of his party's optimistic approach to foreign policy. In 1931, however, his record at the Foreign Office was regarded as the one clear success of a Labour government paralyzed in domestic affairs by the magnitude of the economic collapse.

Wrigley's study reflects its subject; it, too, is solid, plodding, and a bit dull. It is distinctly a career biography, giving scant attention to Henderson's private life. The death of his son in World War I, for example, is disposed of in a sentence, and the author refrains from any speculation as to the effect of this loss on his later activities on the international scene. It is entirely possible, of course, that the materials for a fuller picture of the man simply do not exist. If so, we must despair of ever gaining a more intimate knowledge of this significant figure.

The usual bibliography is replaced by a brief "Note on Sources." Because one of the strengths of this study lies in the author's unearthing of obscure archival materials, it is unfortunate that scholars will be forced to page back and forth through footnotes rather than having the information they seek presented in accessible form.

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JOHN CHARMLEY. *Chamberlain and the Lost Peace*. Chicago: Ivan R. Dee. 1989. Pp. xiv, 257. \$27.95.

Of all the British prime ministers in this century, none was so universally vilified as Neville Chamberlain. As the embodiment of the "Guilty Men," inextricably identified in popular consciousness with Munich and the failures of appeasement, he was dismissed by contemporaries and by subsequent historians as naive and ineffectual, miscalculating Adolf Hitler's intentions and betraying Czechoslovakia. Revisionist historians have undertaken modest reappraisals of several interwar statesmen, but John Charmley's attempt to rehabilitate Chamberlain is ultimately unconvincing. Whereas some writers have attempted to justify appeasement on the pragmatic grounds that it provided Great Britain with breathing space in which to undertake belated rearmament, Charmley offers the dubious hypothesis that Chamberlain's "was the only policy which offered any hope of avoiding war—and of saving both lives and the British Empire" (p. 212).

According to this interpretation, Chamberlain—whose perspective, like that of many in his generation, was shaped by the experience of World War I—had the foresight to recognize that Britain lacked the economic and labor resources to wage another European war. Even in the unlikely event of victory, the foundations of British power would be undermined to the benefit of Soviet expansion. Given these

presuppositions, Chamberlain pursued the realistic goal of tailoring British foreign policy to its limited defense capabilities, which, even at the peak of rearmament, would fail to equip the country for full-scale Continental war. Therefore the main thrust of his policy was toward the lessening of tension in Europe through the achievement of Anglo-German accord. If this implied dismantling the Versailles settlement in the interest of making restitution to Germany, so much the better, since it would establish firmer foundations for a lasting peace.

Such views were hardly in accord with the dominant Foreign Office outlook, which regarded the German menace as axiomatic, or with the anti-appeasers, who chastised the government for yielding to the dictators. Charmley, who discounts Anthony Eden's "natural propensity to take an exaggeratedly moral line in foreign policy" (p. 29) and dissentient MPs as "an odd assortment of rebellious backbenchers and nonentities" (p. 58), seeks to refute the image of Chamberlain as a foreign policy novice, forging ahead on his own in defiance of expert opinion. His evidence reveals not only that the Cabinet remained remarkably united for most of his tenure but also that the prime minister was assiduous in consulting military and diplomatic experts. Yet, as the author indicates, Chamberlain was adept at securing precisely the advice that would confirm his own preconceptions. Thus, he preferred the views of Nevile Henderson, the ambassador to Berlin, who regarded German predominance eastward as inevitable and who consistently counseled in favor of nonintervention in central Europe, to those of the more Germanophobic Foreign Office veteran, Robert Vansittart. Moreover, even the façade of unanimity was shattered by 1939 when Lord Halifax, the foreign secretary, and Alec Cadogan, the permanent head of the Foreign Office, began to dissociate themselves from Chamberlain's determination to prevent war at whatever cost.

If Charmley's portrait of a courageous leader with "a deep and humane desire to leave no stone unturned to avoid war" (p. 95) is persuasive, his defense of Chamberlain's policy is not. Although prescient in his mistrust of Stalin, the inability to recognize Hitler's ambition to dominate Europe by force until 1939 suggests a credulity that staggers the imagination. His assurances to Poland were demonstrably hypocritical, since Britain retained the right to decide the circumstances under which it would intervene. Churchill may have been, as the author claims, an embittered adventurer with fallible political judgment, but he at least perceived that peace on Hitler's terms meant dishonorable surrender. Perhaps that is why Winston Churchill's reputation remains largely untarnished, while Chamberlain's, Charmley's initiative notwithstanding, cannot be resuscitated.

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MARIANNE ELLIOTT. *Wolfe Tone: Prophet of Irish Independence*. New Haven: Yale University Press. 1989. Pp. x, 492. \$35.00.

Wolfe Tone (1763–98) is generally remembered as the leader of the Rising of 1798, the father of Irish republicanism and separatism, and the advocate of armed struggle and blood sacrifice to obtain Irish independence. Marianne Elliott's definitive biography offers us a different memorial. Tone was but one of many United Irish activists, indeed one of the few not arrested before 1798; he was neither father nor founder but a "prophet" of Irish nationalism. In exile from 1795 on, Tone finally returned to Ireland in November 1798, only to be arrested and commit suicide in prison. His grave in the family plot at Bodinstown, County Kildare, had no headstone until 1844. Young Ireland rediscovered him, and in the 1860s the Fenians began to fashion the Tone myth. By 1913, the Gaelic nationalist Patrick Pearse could call Tone's grave "the holiest place in Ireland" (p. 416).

Wolfe Tone is as important as it will be controversial. Using the same scrupulous and wide-ranging international research that characterized her prize-winning *Partners in Revolution* (1982), Elliott has produced a work that is elegantly written and richly detailed. This substantial biography presupposes much specialist knowledge of Ireland and France in the 1790s; readers not so informed will want to do background reading.

Elliott offers important revisions to our understanding of the decade's events. She argues that the Rising of 1798 was not so much dynamic as reactive, a last gasp after four years of government suppression. Tone, who was living in America, France, and Holland, missed the years of suppression and even the rising (May–September 1798). For France, Ireland was always a sideshow. The unsuccessful, storm-wracked sailing to Bantry Bay (December 1796) was already late, and subsequent delays confirmed France's basic disinterest in the United Irish cause. By 1798, Tone was fatalistic and detached. He knew that French aid was too little, too late, and that the Irish revolt was sure to be crushed.

The book's real value, of course, lies in its revision of Tone. Tone spent most of his adult life devoted to domestic reforms, principally Catholic emancipation (half of Elliott's book takes us through 1793; 1797–98 receive seventy pages). Trained to the law, Tone was an articulate writer (and a poor speaker), not a man of action. A middle-class Protestant Dubliner, he hated the landed elite's privilege and corruption. A meritocrat, he was comfortable in cities, favored industry and commerce, and liked many Belfast Presbyterians. Tone came to believe in the illusion that society's producers (Catholics and Dissenters) could ally to replace the Anglican aristocracy and create a new Irish society. But the concepts that came to be associated with Tone's name—armed struggle (he

avored invasion, not guerrilla warfare) and separation from Britain—were ones that he developed only late in life, and he believed that these were thrust on him by Dublin Castle's policies of terror.

Elliott's Tone is a complex man of contrasts and contradictions: Tone, the anticlerical Enlightenment deist who favored Catholic equality (he believed the Catholics were outgrowing their superstitious, pre-modern religion); Tone, the republican who stopped attending United Irish meetings in 1793–94 because of the inflammatory speeches; and Tone, the educated cosmopolite who was appalled by the rough, rural culture of poverty he found on a visit to the west of Ireland. Tone was probably most changed by his exile. America, whose inhabitants he found unsocial and money-grubbing, nevertheless reinforced his republicanism; France, where he was often lonely and always penniless, reinforced his anti-Englishness.

This study reminds us not only of Ireland's international connections in the era of revolution but also, more broadly, of the need to separate man from myth. In demythologizing Tone, Elliott perhaps diminishes Tone's place, but not his importance, in Irish history. Tone was neither a novel or deep thinker nor a national liberator. He was a man of principle with a strong sense of right. Incensed at Ascendancy governance of his country, with a growing realization of England's complicity, he came to believe in the need for extreme measures. Both in Ireland and in exile Tone was one of many decent, ordinary, even obscure persons of principle who protested, got entangled in a spiral of events, eventually took up arms, and ultimately gave their lives for their beliefs. The book is depressing, exhilarating, and engrossing; it is also a major contribution to Irish historical studies.

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BRENDAN O'CATHAOIR. *John Blake Dillon, Young Irelander*. Dublin: Irish Academic Press. 1990. Pp. xii, 211. \$39.50.

Brendan O'Cathaoir, a journalist with the *Irish Times*, has written a good amateur history. The senate of the National University of Ireland has underwritten the volume's publication presumably because it is an expansion of the author's master's thesis. The narrative is straightforward, chronological, and factual. There is no theory, little interpretation, and no flights of the imagination. Based on the Dillon papers at Trinity College, Dublin, the story is well documented and is admirably open to checking and replication.

John Blake Dillon deserves the attention of Irish historians both because of his own influence on the Young Ireland movement and because he was the father of John Dillon, chairman of the anti-Parnellites and, ultimately, the last head of the Irish nationalist

party. John Blake Dillon (1814–66) came from a stratum of Irish society that usually is forgotten: those Roman Catholics who remained comfortably middle class or gentry despite the eighteenth-century penal laws.

Dillon's father held about 150 acres of land in the west of Ireland and, despite a family tradition of being a rebel in 1798, he actually was a member of the local yeomanry and had taken part in the rout known as the Castlebar Races. The young Dillon was privately tutored and after two years at Maynooth College entered Trinity College in Dublin, then a Protestant establishment. Thereafter he studied law in London and was admitted to the bar in Ireland.

Dillon's claim to being a part of the genealogy of Irish patriotism rests in large part on his friendship with Thomas Davis and in their joint founding of the *Nation* newspaper. Dillon took a significant role in the rising of 1848 and thereafter escaped to the United States. The government permitted him to return to Ireland in 1856. There he practiced law and prospered to the extent of having both a large townhouse on Fitzwilliam Square and a seaside lodge at Killiney. In 1865 he was elected a nationalist, anti-Fenian M.P. for Tipperary. His political career was suddenly cut short by his death the next year.

This volume will be useful to future scholars. If, unfortunately, it gives the reader no sense of what made Dillon tick, other scholars can do that. The basic facts are laid out here, and for that O'Cathaoir merits credit.

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JEAN-MICHEL MEHL. *Les Jeux au royaume de France du XIII^e au début du XVI^e siècle*. (Nouvelles études historiques.) Paris: Fayard. 1990. Pp. 631. 198 fr.

The word "game" encompasses an antinomy, and therefore the historian of games must make sharp choices of method and procedure: no game takes place until rules and limits are accepted, yet the reason for a game is to "play," to fool around, to indulge one's hopes and whimsy and skill to the point of moving beyond the game's acknowledged limits by playing with such subtlety or verve that rules are subordinated to professional finesse and personal pleasure. Is what interests historians, then, the game as a set of rules, differing over time in players, paraphernalia, and social acceptance? Or is what engages them rather the game as a psycho-social proclivity, as an instance of the playful, symbolizing, mimetic faculties in human nature? Johan Huizinga's *Homo ludens* (1938) set the agenda for the latter kind of historical inquiry. In a recent collection of about forty articles by an international group of scholars, this agenda was dominant in well over two-thirds of the articles: the meaning and repercussions of games

and the modes of their representation in literature and art were given more importance than the identity and perquisites of gaming procedures (*Les Jeux à la Renaissance* [1982]).

While posing in passing many of the currently fashionable questions of game interpretation, Jean-Michel Mehl has designed his research primarily with reference to behaviorally measurable goals: what games were played, by whom, and how they were played in France between the thirteenth and the early sixteenth centuries. To answer these questions within reasonable compass Mehl restricts the meaning of the term "game" sharply. Sports and games imitating or educating for the gestures of war are generally omitted, although he does describe archery and crossbow competition and one of the ancestors of soccer and football, *soule*. Games designed for festive purposes and mummeries of every kind, let alone farce and drama, are also out of bounds. Playful kinds of games and playful aspects of games are avoided in this inquiry as far as can reasonably be done.

Mehl's preferred sources are the pardon letters issued by French kings and some feudal princes. They, together with the household accounts of a number of feudal princes, and especially of the dukes of Burgundy, allowed analysis of 1,619 occasions for which there was enough information to name the game, its participants, and its outcome. This corpus of occasions is well analyzed in the second section of the book, which is devoted to sociological questions: what classes took part in what kind of games (for example, chess predominantly among nobility, handball among urban and noble classes, dicing among all classes), when (the influence of calendrical customs), where (the gradual emergence of specialized *maisons de jeu* in the fifteenth and sixteenth centuries, and the predominance of tavern and marketplace locales), and for what stakes (typical monetary stakes and failure to pay and modalities of cheating and violence). One-fourth of the book is concerned with these distinctions (part 2) while nearly one-fifth of it describes legal and moral discourses about the variety of games (part 3).

In keeping with its objectivizing goal and half-quantifying, half-descriptive method, part 1, the longest section of the book, includes descriptions of each popular pastime, which in many cases are illustrated with woodcuts and manuscript reproductions. The rules and character of each game, some information about its probable geographic and temporal origins, and its characteristic equipment (from playing cards to lacrosse sticks) are inventoried. Part 4 of the book, short and synthetic, adds little to preceding sections. During these four centuries games evolved toward greater complexity and thrived in all social milieus in spite of state and ecclesiastical thunder (chiefly directed against dicing).

The book is a mine of information about particular subjects. For that reason the lack of a substantive index cuts in half the volume's chances for usefulness

and recognition. The bibliography is broken into thirty-three topics and subtopics that seem to reflect the accents of the author's research. Such listing insures the reader's despair when trying to locate an "op. cit." buried in one of the book's more than two thousand endnotes.

SAMUEL KINSER

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HENRY HELLER. *Iron and Blood: Civil Wars in Sixteenth-Century France*. Buffalo, N.Y.: McGill-Queen's University Press. 1991. Pp. xiv, 191. \$34.95.

In this informative and combative interpretation of the violence that wracked the sixteenth century, Henry Heller perceives a pattern of class conflict that spanned the century. Consequently, he wants "to link the popular protest of the earlier decades of the century with the Calvinist Revolt, the ensuing Catholic League and the revolts of the 1590s" (p. xi) and in so doing to gain "insight into the system of domination" (p. 36) that weighed heavily on this unhappy century.

As one might expect, the book is divided into two chronological phases, the first concerning the popular revolts of the 1520s and 1540s. These disturbances were marked by the deterioration of the economic position of "the majority of commoners" (p. 14) brought on by increases in food costs, rents, and taxation, but significantly for Heller they also held in germ a "democratic view of society" (p. 36) that opposed the commoners' political exclusion from the existing oligarchic system. These revolutionary political aspirations, according to Heller, would be more prominently displayed in the revolts of the century's second half.

Eschewing religion as an essential motivation during the "Civil wars" (more conventionally known as the Wars of Religion), Heller argues that the revolts that erupted sporadically from the 1560s to the 1590s wore religious disguises. Democratic radicalism was cloaked by militant Calvinism in the 1560s and Counter-Reformation Catholicism in the late 1580s and early 1590s. The real rebellious aspirations of this second phase—a democratically inspired "repudiation of the nobility"—were only unveiled in the revolts in the Dauphiné and the Vivarais in 1579–80 and by the Croquants in the mid-1590s. Indeed, the "noble class" was as aware of the radical nature of the political and economic aspirations of the commoners as the plebeians were, and responded with a counter-attack. In fact, for Heller the civil wars that raged off and on for thirty years or so were at bottom a war waged by the nobles against the rest of society, a "noble reaction" with "counter-revolutionary" (p. 58) intentions in which "religion [was] beside the point" (p. 60).

To support his thesis that the salient link between the popular revolts of the first half of the century and

those of the second half was class rather than confessional antagonism, Heller is forced repeatedly to banish religion from the range of motivations that contributed to the violence of the period. In so doing he flies in the face of much recent scholarship (for example that of Denis Crouzet, *Les guerriers de Dieu* [1990]). But even if we grant Heller that class conflict may be a useful heuristic device explaining the violence, we still must grapple with the connections between labels and groups of historical actors. Here Heller offers little help. True, in broad strokes one can perceive, as Heller does, a line of battle—the dominators versus the dominated—but, as numerous local studies have demonstrated, close examination of that line of battle finds it exceedingly difficult to keep contestants arrayed in one camp or the other. And Heller's sweeping categories are so imprecise and interchangeable (and laden with historiographical baggage) as to be nearly meaningless. Commoners, plebeians, the people, craftsmen, peasants, lesser merchants, the middle class, the bourgeoisie, the lesser bourgeoisie—all are at one time or another in Heller's account in rebellion against nobles, notables, patricians, great merchants, *officiers*, the church, the king.

Still, despite its methodological shortcomings, this archivally rooted book finds a useful place in the literature of rebellion by bridging the well-known late-medieval revolts on the one side and the equally oft-studied rebellions of the seventeenth century on the other. One cannot help but come away from this work realizing that France's supposed "Golden Age" gave way to one of bloody iron considerably sooner than historians have traditionally granted.

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PIERRE GOUBERT. *Mazarin*. Paris: Fayard. 1990. Pp. 572. 150 fr.

A new biography of Cardinal Mazarin by a historian as great as Pierre Goubert is indeed an event. Connoisseurs of the French intellectual scene may be amused to find the doyen of the *Annales* school, demographer and quantifier *par excellence*, writing *histoire événementielle* in Fayard's highly successful collection of biographies aimed at the general public. But a reevaluation of the centrally important period 1643–61, which has frequently been treated only as an unraveling of Cardinal Richelieu's achievement or as a negative lesson for the young Louis XIV, is long overdue. Much of the blame for this alleged failure has been heaped on Mazarin himself, and Goubert attempts to redress the balance by writing a straight political biography, giving much less primacy to demographic environment and material conditions than he did in his earlier, more innovative, study of Louis XIV.

Goubert traces Mazarin from his early days in

Rome through his various diplomatic missions, his rise to power, his skillful international maneuverings, his handling of the Fronde, and his rarely appreciated accomplishments from 1653 to 1661. There is also discussion of his fortune, his art collection, and his training of Louis XIV. All of this is narrated with the author's inimitable clarity, skepticism, and wit. He gets to the heart of each issue rapidly, tearing away illusions, putting new findings into perspective, going over ground he has covered many times before but with the fresh eye of the experienced master. The thrust of his argument is that without Mazarin, Richelieu's accomplishments would be forgotten and Louis XIV would not have triumphed. "I will attempt to react, no doubt in vain, against the mad cult of the Great Reign and the Great King, which has again become the mental disease that one thought had been put to rest" (p. 14). Is this a comment on Fayard's successor volume in the series, François Bluche's *Louis XIV*? The linchpin of French seventeenth-century hegemony was Mazarin's extraordinary talent, fueled by and linked to the underlying wealth of the country itself. Mazarin was a brilliant diplomat and a skillful manager of persons and factions. He did not fully grasp the complexities of Paris, but he had military force and the king's approaching majority on his side and the rest was just a matter of patience, skillful maneuvering, and charm. Money was also central. Goubert incorporates the recent findings of Françoise Bayard and Daniel Dessert concerning the role of financiers as the link between the wealth of the country and the needs of the state. The deficit that paid for the royal armies was covered by mortgaging future revenues to cartels of investors who in turn were backed by the investments of the great nobility, the officer class, and the merchant bourgeoisie. Mazarin was thus piloting a ship with everyone of importance on board and, viewed in this light, Goubert makes the conflicts of the Fronde look more like petty squabbles than like revolutionary challenges.

The merit of this book is the sensible, coherent picture that it provides of the broad continuity of government policy. But this is also its weakness. In presenting things from the Cardinal's point of view, Goubert is pulled so decisively toward sympathy for the king's and the Cardinal's state-building mission—sympathy curious in one so negative about Louis XIV—that he seriously underplays the internal problems of the era: provincial rebellion (only Paris matters), social dislocation (there was misery but the peasants were resourceful), the Fronde (wait for the factions to quarrel among themselves). Even the rebellions of Bordeaux and Provence are reduced to the status of temporary inconveniences. In the process much of the crisis that explained and conditioned the period is lost. This is a valuable, engaging synthesis but it contains few surprises.

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ALICE STROUP. *A Company of Scientists: Botany, Patronage, and Community at the Seventeenth-Century Parisian Royal Academy of Sciences*. Berkeley and Los Angeles: University of California Press. 1990. Pp. xv, 387. \$49.95.

The premier scientific institution of its day, the French *Académie Royale des Sciences* (1666) forged new ties between modern science and the state. The definitive history of the Academy before its reform in 1699, however, has yet to be written. Drawing extensively on archival sources and incorporating her extraordinary, previously published analysis of its finances, Alice Stroup here presents the most thorough study to date of the early Paris Academy.

Stroup reveals a fledgling institution negotiating its own autonomy between the Scylla of practical applications demanded by its patrons and the Charybdis of pure scientific investigation toward which academicians inclined. A fifty-page appendix enumerates the Academy's accounts, and Stroup documents the changing fortunes of the institution under Jean-Baptiste Colbert, who supported it munificently; the marquis de Louvois, who cut funding, demanded practical benefits, and interfered directly in research programs; and L. P. de Ponchartrain, who, despite received notions to the contrary, provided for the Academy under strained wartime conditions. Stroup provocatively interprets the Academy as a mercantilist company trading in knowledge for the crown, and she rightly concludes that government support for the Academy represented new forms of patronage for science and a key step in the development of the modern professional scientist. Stroup's evocations of seventeenth-century scientific Paris and the Academy's international contacts are especially rewarding. Throughout, Stroup's wonderful command of detail makes the Academy come alive, as, for example, when she writes about the Academy dissecting deceased members, or about highwaymen stealing an academician's manuscript. This volume is handsomely illustrated and thoroughly researched, but the "conclusion" ending each chapter too often is a mere summary.

Stroup focuses on botany in the Academy and does not treat the institution's more substantial theoretical and practical work in other sciences. Although hers does not pretend to be the sought-after definitive history of the early Academy, the emphasis on botany entails two serious problems. First, Stroup provides little rationale for singling out botany, beyond the observation that botany "may serve as a barometer of the institution as a whole" (p. 61). Consistently characterizing the Academy's botanical program as a failure, she does not show the relevance of the botanical case or how it can illuminate the Academy's larger successes in astronomy and cartography, for example. Second, Stroup devotes over one hundred pages (40 percent of her text) to an elaborate reconstruction of the "internals" of the Academy's botanical re-

search, including a formidable historiographical exegesis of the role of "analogy" in seventeenth-century science. Other experts in the history of botany per se will doubtless find these chapters of great interest. A separate chapter on botany and medicine shows that Stroup can treat internal science (in this case ergotism) deftly to reveal the workings of science in an institutional context, but her extensive internalist treatment of botany is not required to support the modest conclusions drawn in a final five-page chapter on the Academy as an instrument of the crown.

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E. J. HOBBSAWM. *Echoes of the Marseillaise: Two Centuries Look Back on the French Revolution*. (Mason Welch Gross Lecture Series.) New Brunswick, N.J.: Rutgers University Press. 1990. Pp. xv, 144. Cloth \$27.00, paper \$9.95.

For many historians the bicentennial of the French Revolution was a celebration of the triumph of revisionism, the end of the long hegemony of the Marxist or Communist interpretation (the two are intertwined in France). E. J. Hobsbawm reminds us that the corpse has not yet been laid to rest, that revisionism is not indisputably victorious, and that the "historic significance and transforming capacity" of the French Revolution are "utterly obvious" (p. 109), however minimized by revisionists.

Hobsbawm's strategy is to describe the refraction of the French Revolution "through contemporary political prisms" (p. 69). His first chapter considers what "the nineteenth-century Liberal bourgeoisie made of the French Revolution"; his second chapter examines "those who feared, or made, or hoped to make" a revolution (p. 67); his third explores the use of the revolution in post-revolutionary Russia; and the last chapter looks at "surviving revisionism."

Hobsbawm takes seriously the cliché that "everybody had his or her French Revolution," which "depended not on the politics and ideology of 1789 but on the commentator's own time and place" (p. 69). Contemporaries of the French Revolution and the following generation saw it as a bourgeois revolution, "the culmination of the secular rise of the bourgeoisie to the position of ruling class" (p. 16). The revisionist view that the revolution was unnecessary or that the bourgeoisie was not a clearly identifiable class "belongs to the discourse of the late twentieth century" (p. 24). How the revolution was seen, how it affected action, transcends the "intellectual provincialism and tunnel vision" and "monographic myopia" of revisionists (p. 112).

The question of Marxism is more complex. Hobsbawm makes the points—which François Furet has also made—that what has come to be called the Marxist interpretation of the French Revolution owes

more to Jacobinism and Republican historiography than to Marx (p. 77), and that Robespierre's predominance in recent Marxist historiography has "little to do with Marx and Lenin" (p. 54). The French Left, especially the Communists, have attached their Jacobin interpretation to Marxism, thus conflating political and historical discourse: to attack the one is to attack the other.

The most interesting section, however, is Hobsbawm's discussion of interpretations of the French Revolution in Soviet Russia. The struggle between Trotsky and Stalin, so crucial for subsequent history (and historiography) was argued in the language of Thermidor and Bonapartism (p. 59). This name-calling, this self-conscious historical reference—even likening the Stalin trials and purges to the French Terror (p. 54)—should caution against "an excessive tendency to look for history to repeat itself" while showing how deeply immersed the Russian revolutionaries were in the history of their predecessors (p. 59). Hobsbawm sums up his discussion of Russia by making another point Furet also insists on: it took the French almost a century to achieve "a democratic parliamentary republic." We may assume that Lenin in 1923 foresaw the same "lengthy historical process" (p. 65).

This is a vigorous, refreshing, and learned brief on behalf of a venerable historiographical tradition. It reminds us of the obvious but often overlooked truth: that there are no definitive interpretations, certainly not of an event so primal and transcendent as the French Revolution. It is premature and intellectually irresponsible to gloat over revisionism's momentary triumph.

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SUZANNE DESAN. *Reclaiming the Sacred: Lay Religion and Popular Politics in Revolutionary France*. (Wilder House Series in Politics, History, and Culture.) Ithaca: Cornell University Press. 1990. Pp. xv, 262. \$33.50.

Catholic, republican, and Marxist historians have stressed for over a century the mutual antagonism, if not the total incompatibility, of Christianity and the French Revolution. Noteworthy exceptions are Bernard Plongeron (1973) and Timothy Tackett (1977, 1986), who in different ways have delineated a "third way" between counterrevolutionary Catholics and dechristianizing republicans. Whereas their works focus mainly on the clergy, Suzanne Desan has discovered a mine of documents on lay Catholics in the Yonne in northern Burgundy. She offers these as a test case for what could be called a Catholic-republican thesis in one department of France (not, as its title suggests, for the whole nation).

After a solid but unexceptional historiographic introduction, Desan provides an informed back-

ground to the geography, economy, and society of the Yonne. The next three chapters take up lay cults, the "rhetoric and politics of religious revival," and female religious activism.

The point Desan somewhat repetitiously drives home is that the Yonne between 1794 and 1799 experienced a true religious revival of remarkable proportions. While revival of some sort can be found elsewhere in France in the years following the Terror (and Desan does relate it to movements in other regions; see pp. 158–164), the originality of the Yonne revival lies in the fact that it was republican. Men and women there demanded their rights, particularly the right of religious liberty, not as counter-revolutionaries but as citizens attached to 1789, their rights to the tricolor, to the Declaration of the Rights of Man and of the Citizen, and particularly to the idea of popular sovereignty (especially as it challenged ecclesiastical hierarchy). The believers of the Yonne wanted not only the religious liberty denied them in the recent past but also rioted to have their churches reopened and actively opposed the imprisonment or deportation of their priests. Protesting the removal of church bells during what Desan rightly calls the second Terror after the coup of Fructidor (September 1797), the parishioners of Mont-Saint-Sulpice denounced the "tyranny" and "despotism" of the authorities (p. 139). Revolutionary rhetoric was thus turned against republican officials to force them to comply with the wishes of the people, but not, as in the west of France, to unravel the revolution.

A second important point, Desan argues, is that when the people of the Yonne were "reclaiming the sacred" they usually did so without a priest. They said "white masses" and administered most of the sacraments, thus replacing a clerical and hierarchical religion with a people's church *avant la lettre* (the pope does not figure at all here). It is, however, not certain whether lay people did this because circumstances deprived them of priests (pp. 90, 92, 221) or because they were committed to a priestless cult.

Many of Desan's examples of activists petitioning for the release and return of curés (pp. 128, 133, 135, 149, 152, 168) undermine her interpretation of a totally lay cult. Lay people may have simply stood in for absent priests rather than seeking to replace them permanently. There were certainly many priests left functioning in the Yonne as well, in a large majority of its cantons by Desan's own count (p. 90), but we hear nothing of them. The lay cults of the Yonne were most likely a temporary expedient lasting only as long as that situation endured (into the nineteenth century). Such an interpretation is bolstered by the fact that lay people in revolutionary Yonne attempted neither to consecrate the host nor to hear confessions. Evidently they respected some limits as to how far they could penetrate the sacred, something which Desan seems to overlook.

Desan explores the role of other important factors in the Yonne: the high subscription to the 1791

Ecclesiastical Oath accepting the civil constitution of the clergy (85 percent); strong Jansenist precedents for secularity; and the Catholic Reformation's unsuccessful attempt to repress some popular devotions. All three were important influences on the lay cults of the 1790s. Desan depicts convincingly the involvement of lay women in defending Catholicism, and her explanations seem plausible. For example, the bread-procuring role of women in the marketplace was perpetuated in their new role as the provisioners of "sacred" bread in the lay cults. One wonders, nonetheless, whether the cult of lay women and men in priests' vestments Desan has discovered is more genuinely secular than one that honors a lay sphere apart from the clerical world.

Even if Desan has overstated the case for a lay religion in the Yonne, her argument for a lay Catholic republicanism seems to me indisputable. Her text is smoothly written and well informed on such intricate matters as the relationship of the Parisian to the departmental revolution. If one is unsure about the exact mix of religion and revolution on a national scale, Desan's work is there as a challenge.

R. EMMET KENNEDY, JR.
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W. D. EDMONDS. *Jacobinism and the Revolt of Lyon, 1789–1793*. New York: Clarendon Press of Oxford University Press. 1990. Pp. xi, 349.

In the history of the French Revolution, the city of Lyon holds a reputation as an early center of counterrevolutionary conspiracy, and as the site in 1793–94 of the bloodiest repression of the Terror. In this thorough and sensitive book, William D. Edmonds sets out to debunk the notion that Lyon was a hotbed of royalist intrigue and to explain the sequence of events that led the city to revolt against the National Convention in 1793, a revolt that brought in its wake a siege, the Terror, and finally the infamous pronouncement that "Lyon is no more."

The book is essentially a study of Lyonnais politics during the first five years of the revolution. Edmonds discusses Old Regime politics in Lyon, of which there was very little, and various aspects of the Lyon economy and social structure on the eve of the revolution. In particular, he emphasizes the dominance of the silk industry in the city, with the accompanying sharp tensions between the capitalist silk merchants and their workers and the general economic slump that Lyon experienced in the 1780s due to a decline in demand for silk products. Although silk dominated the Lyonnais economy, silk merchants did not dominate Lyon politics, either before or during the revolution. The propertied classes generally were a divided lot, Edmonds argues, and that lack of cohesiveness meant that there was no municipal revolution in Lyon in 1789–90, as there had been in so many other towns. It also meant that in subsequent

years there would be an opening for the popular classes in local politics. Indeed, Edmonds shows that an organized popular movement existed in Lyon before it did in Paris.

That popular movement took root in the political clubs that formed in virtually all thirty-four sections of Lyon in 1790–91. Lyon radicals (Edmonds calls them *patriotes*) used the clubs to launch electoral campaigns, probably even a little electoral fraud, and gained control of the municipal council in late 1792. At this point, Lyon's propertied elite saw their interests and social position threatened. Edmonds follows the lead of Colin Lucas ("Nobles, Bourgeois, and the Origins of the French Revolution," *Past and Present* 60 [1973], 84–126) here, arguing that it was this challenge from the lower classes, rather than opposition to the aristocracy, that gave the bourgeoisie of Lyon a sense of class consciousness.

Once in power Lyon's Jacobins, whose own social base of support was very narrow, managed to alienate a good portion of the popular movement by their overly aggressive social policies and by their efforts to consolidate power through centralization of the political process. In the spring of 1793 the moderate bourgeoisie appealed to a tradition of sectional independence and the ideal of popular democracy (somewhat hypocritically) and launched a successful revolt against the Jacobin municipality.

The revolt of the sections in Lyon came just on the heels of a similar uprising in Marseille, and on the eve of the May 31–June 2 revolt of the sections in Paris. Edmonds points out some interesting differences between sectional politics in Lyon and Marseille (they seem rather more independent and deliberative in Lyon), and deftly unravels the interconnections between the local revolt and Lyon's involvement in the "federalist" revolt against the Montagnard Convention. With Edmonds's book, we now have serious studies in English of all four of the major "federalist" centers (Lyon, Marseille, Bordeaux, and Caen).

A short review can scarcely do justice to this complex and multifaceted work, but neither would any review be complete without a few quibbles. There are a number of fine maps in the book, but nowhere do we find a decent key; the reader who has not been to Lyon will find it difficult to sort out the urban and social geography of the city, which Edmonds rightly asserts is very important. The very valuable social analysis of Lyon and its neighborhoods, made early in the book, is not carried through into later chapters as fully as it might be, although Edmonds never loses sight of social factors. Finally, there is much valuable information in the dozen appendices, some of which might have been incorporated into the text. These points aside, this is a fine work in which all serious students of the French Revolution will find much to learn.

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JUDITH WISHNIA. *The Proletarianizing of the Fonctionnaires: Civil Service Workers and the Labor Movement under the Third Republic*. Baton Rouge: Louisiana State University Press. 1990. Pp. x, 394. Cloth \$47.50, paper \$16.95.

This book, dedicated "aux camarades des PTT," is a sympathetic but scrupulous study of lower-level state employees (such as teachers and postal workers), whose labor movement became increasingly identified during the Third Republic with that of blue-collar workers in France's Confédération Générale du Travail (CGT). Beginning the story with the illegal unions of the late nineteenth century, Judith Wishnia traces the evolution of *fonctionnaires'* militancy and organization through the end of the Third Republic (1940). While readers may regret not learning more about recent developments, they should be happy to see Wishnia transcend the usual periodization, so rarely fruitful in social history, that begins or ends with 1914.

Although not officially allowed to unionize until 1946, French *fonctionnaires* formed legal "associations" in 1901 and fought for broader rights as they saw their early advantages of status, pay, and job security erode over time. This "proletarianization" made the "employer-state" seem no less exploitative than any private employer and lessened differences between *fonctionnaires* and blue-collar workers (pp. 3-4); the CGT's own increased reformism, as its white-collar membership grew, further closed the gap. Thus, the *fonctionnaires* joined the CGT after World War I; withdrew into autonomy after the schism of 1922; rejoined (in most cases) the rump CGT in 1927; and in 1936 became the principal force behind the CGT's reunification and Popular Front politics. This pro-Left, pro-labor orientation clearly distinguished the French *fonctionnaires* from the German *Mittelstand*, whose slide into Nazism implied both a different social identity and a greater devotion to the authoritarian state.

Still, the *fonctionnaires'* proletarian identity had its limits. Even while demanding the right to unionize, many civil servants—especially teachers—regarded the right to strike as inapplicable to them, however intimately "strike" and "union" (or "revolution" and "syndicalism") remained linked in the public mind. This ambivalence or blurred identity is carefully noted by Wishnia, although the question of solidarity across class or occupational lines might have come into clearer focus if more were shown about relations between the "*petits fonctionnaires*" and the higher-level civil servants who did not join the CGT.

Wishnia's research also relies heavily on speeches and writings of *fonctionnaire* leaders, so that little is learned about rank-and-file attitudes (admittedly hard to glean from the customary sources). Statistical analysis of personnel dossiers, or study of the wage hierarchy within or among professional categories, might have buttressed a sociological interpretation of

types or degrees of solidarity, described here more than explained. The increase in female civil-service work, for example, is viewed here as a source of feminist consciousness but otherwise scantily treated, with few comparisons to women's employment or unionization in other occupational groups.

These quibbles aside, Wishnia's unabashed exercise in "labor history from above" illuminates an unfamiliar occupational sector and links it effectively to the CGT's larger history. The *fonctionnaires'* hardships also highlight what Wishnia rightfully calls the "tragedy" (p. 225) of repeated labor schism in France. However alien this endemic disunity may make the French case to other national experiences, the post-World War II explosion in state employment, plus civil servants' special vulnerability to state-imposed austerity programs, make the *fonctionnaires'* story a valuable one for labor historians of all geographic specialties.

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GABRIELLA FIORI. *Simone Weil: An Intellectual Biography*. Translated by JOSEPH R. BERRIGAN. Athens: University of Georgia Press. 1989. Pp. x, 380. \$35.00.

In 1973, French philosopher Simone Pétrement published a remarkable two-volume biography of her old friend Simone Weil, who had died forty years earlier. Although there had been several efforts at sketching out Weil's remarkable life before, Pétrement's work convincingly demonstrated how Weil's astonishing asceticism grew both directly from her thinking as well as from the unusually altruistic and empathic bent of her personality since she was a child. Recently there has been a plethora of serious monographs on Weil: Peter Winch on her philosophy, L. Blum and A. Seidler on her political theory, and David McLellan on the juncture of life and thought. Perhaps one reason for the contemporary interest in her thought is the way in which, as she would not have been surprised to note, the most effective challenge to communism in Eastern Europe proved to be religious in inspiration.

Gabriella Fiori, who published this prize-winning study in Italian in 1981 and lectured on Weil throughout Europe, has helped fill in the picture, particularly by hunting out new sources and publishing a few negative or nuanced judgments that previous adulatory biographies might have passed over. Although often revered as a sort of pilgrim-saint or holy fool, Weil is shown to have been unattractive, even irritating, to many of the workers she sought with her intense solicitude (p. 47). Perhaps the greatest contribution of this work is the thoughtful analysis of Weil's troubled relationship to her own femininity.

Weil, like her *maître* Alain, had a genius for formulating the pithy aphorisms that make up a large—perhaps too large—portion of this text. But what

biographer could resist including so many brilliant, original, and intransigent reflections on such a range of topics?

Noticeably absent from this, as from other studies of Weil, is adequate attention to the late 1930s political context in which she formulated some of her most striking insights. We learn new details about her close ties to people such as Auguste Detoeuf, Denis de Rougemont, Gaston Bergery, and the *Nouveaux Cahiers* group during the period of her intense interest in factory work, but we are not told about the anti-*Munichois* acceptance of inevitable German hegemony in that circle and its ties to the subsequent *Révolution nationale* under Pétain. We learn more about Weil's wartime stay with the vine grower Gustave Thibon (p. 196) but are not told that Thibon was also considered an important *Révolution nationale* moralist at the time (p. 196). Certain hard but central questions, discussed in some of the aforementioned studies, are downplayed or completely avoided here, such as Weil's adamant denial of her Jewishness to Vichy authorities and, particularly, the alleged Catharist suicidal anorexia that led to her tragic death in England in 1943.

This translation, although much superior to the disastrous rendering of a truncated version of the Pétrement study into English a few years ago, has a few curiosities (Weil is described as stirred by the shouting and "the Charlesons" during a Baptist service in Harlem! [p. 235]). This is a useful and interesting complement to the other new studies on a brilliant, paradoxical, haunted, and haunting woman whom Albert Camus once called "the only great spirit of our time."

JOHN HELLMAN
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TYLER STOVALL, *The Rise of the Paris Red Belt*. Berkeley and Los Angeles: University of California Press, 1990. Pp. xvi, 249. \$37.50.

This exemplary monograph explores one of the more curious ironies of French social reform. Throughout much of the nineteenth century, social Catholics advocated policies to encourage rural home ownership by workers as a means of pacifying them. Yet when such resettlement actually occurred (without public sponsorship) in the Parisian suburbs after World War I, it produced an angry, class-conscious work force inclined to vote communist. Tyler Stovall examines communism in the stronghold of Bobigny with acuity to interpret the creation of a "red belt" around the capital in the interwar years.

Bobigny experienced startling growth after 1900, based not so much on the influx of industry as on the arrival of working-class commuters. Seventy percent of the commune's population consisted of wage earners, but they were workers of a special sort. Escaping the high cost and inadequacies of Parisian housing, these laborers purchased unimproved lots in the

still-open fields of Bobigny and often constructed their own homes. The absence of nearly all amenities and the land developers' quick-profit schemes turned the proverbial dream of home ownership into a nightmare. The residents willingly made the town a secure fortress of communism in a France that as yet gave the party little more than 10 percent of the vote.

Stovall convincingly demonstrates that Bobigny's communist mayor remained loyal to the idea of relentless class struggle articulated in Moscow while serving his property-owning constituency. Skilled leadership achieved both symbolic challenges to the bourgeois state and effective sewer-line socialism. Central to this success, Stovall argues, was the creation of a popular culture of communism among the residents. The people of Bobigny readily identified with exploited workers. They were proud to participate in a grand communist experiment: laborers achieving decent living conditions through their own efforts and within their own citadel. The party not only set the tone for communal festivals but also directed the neighborhood associations that fought for local amenities.

Stovall's portrait is deftly executed. He succeeds in emphasizing heretofore unappreciated aspects of interwar communism: its local flexibility and willingness to shift from workplace to residential issues. The study reinforces the recognition gaining force among labor historians that proletarianization is not a fully adequate explanation for class consciousness. Despite these achievements, some aspects of the problem elude a purely local study of this sort. Why, for example, did the people of Bobigny fail to develop the "house-proud" attitudes so common among working-class homeowners elsewhere? Why the hostility to the capitalist order and the pride in communist solidarity when others might have delighted in rising property values? The residents of Bobigny did not interpret their world from an individualistic perspective. A complete explanation for the culture of communism must include references to an alienating political heritage and to experiences during World War I. These qualifications aside, Stovall uses small, distinctive Bobigny to excellent effect in shedding light on class relations in interwar France.

LENARD R. BERLANSTEIN
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FRANÇOIS-GEORGES DREYFUS, *Histoire de Vichy*. (Vérités et légendes.) Paris: Perrin, 1990. Pp. 818. 195 fr.

François-Georges Dreyfus intends to refute the anti-Vichy "vendetta" (p. 287) of this reviewer, Jean-Pierre Azéma, Yves Durand, Marc Ferro, and others. A prolific historian of Alsace-Lorraine and Germany, Dreyfus describes himself as "of Jewish origin, politically engaged in Gaullism since 1947" (p. 11) (that is, a Gaullist of the postwar Rassemblement du Peuple Français [RPF] rather than of wartime Free France).

He was for years RPF assistant-mayor of Strasbourg, where he is a professor. In 1944 he was hidden by a French family, to whom he dedicates this book.

Although he considers Pierre Laval's collaboration after 1942 a "dishonor" (p. 785), he finds much in 1940–42 that was not so bad, especially if placed "in its context and in its time" (p. 287). For example, Vichy's administrative purge of Freemasons, Jews, and Popular Front supporters in the fall of 1940 was "much smaller than what happens when a President changes in the United States" (p. 263), as if it were only a matter of numbers.

Dreyfus follows Robert Aron's *Histoire de Vichy* (1955) in contrasting an *attentiste* Philippe Pétain with a collaboratorist Laval. Laval's removal on December 13, 1940, thus marks for Dreyfus a victory of the anti-German traditionalists; in contrast, Laval's principal biographers, Fred Kupferman and Geoffrey Warner, attribute Laval's fall to his parliamentary past, his failure to consult colleagues, or perhaps even his failure to deliver the German concessions he kept promising. The opportunist Darlan had, in Dreyfus's version, virtually sided with the Allied camp by April 1942—but this makes his military resistance to the Allied landing in North Africa in November 1942 unnecessarily mysterious. One will want to consult those two masters of the archives, Eberhard Jäckel (*La France dans l'Europe de Hitler* [1968]) for Germany and Jean-Baptiste Duroselle (*L'abîme* [1983]) for Vichy, for a better-grounded interpretation of Vichy's goal as neutral within a German-dominated Europe.

Dreyfus has not made major archival discoveries. He has reworked the secondary material that suits him best, including uncorroborated postwar reminiscences. To give only one example, he claims without citing a source that Vichy's Armistice Army camouflaged enough material for four divisions, including about a thousand tanks transformed into tractors (p. 590). These figures vastly exceed any previously known documentation or serious oral testimony.

Dreyfus argues that if the French government had moved to Algiers in June 1940, the Germans would have seized North Africa, thereby winning the war. Thus, the armistice was a good thing. Dreyfus claims that even Winston Churchill admitted as much to General Alphonse Georges in 1944 (p. 162). This bit of uncorroborated hearsay, a staple of sympathetic accounts of Vichy, flies in the face of every authentic Churchillian statement and action. Moreover, since Hitler proved unable to cross the Channel, it seems unlikely that he could have crossed the Mediterranean. Dreyfus bases his case for a Vichy "double game" in part on an alleged secret agreement between Churchill and a Vichy emissary, Louis Rougier. This account has been decisively refuted by the archival researches of R. T. Thomas (*Britain and Vichy* [1979]).

Finally, Dreyfus's assertion that Vichy "contributed considerably to diminish human losses as the neigh-

bors of France experienced them" (p. 785) needs a more rigorous demonstration. It is indeed surprising to see Dreyfus resurrect the old claim of Vichy's Commissioner for Jewish Affairs, Xavier Vallat, enunciated in Vallat's postwar trial, that since only 75,000 Jews taken from France perished in the Holocaust, a far lower percentage than in Holland or Belgium, Vichy must have done something right. Such a case requires genuine comparison not merely of the final figures but of the actions of all governments concerned, as well as the differing opportunities for escape. Dreyfus attributes Vichy's first anti-Semitic law of October 3, 1940, to a desire not to leave this terrain to the Nazis and not to establish different rules for the two zones, while admitting, without explanation, that the French text was more severe than the German text adopted six days earlier for the Occupied Zone. An alert reader will be skeptical, therefore, when Dreyfus tries later to credit Vichy with saving those Jews who survived the German occupation of France.

ROBERT O. PAXTON
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RICHARD L. KAGAN. *Lucrecia's Dreams: Politics and Prophecy in Sixteenth-Century Spain*. Berkeley and Los Angeles: University of California Press. 1990. Pp. xiii, 229. \$24.95.

In 1980 Richard L. Kagan made the sort of archival discovery about which most historians only dream. In Madrid's Archivo Histórico Nacional he came upon the extensive documentation surrounding the case of Lucrecia de León, a young woman whose bizarre, apocalyptic dreams landed her in the prisons of the Inquisition in the 1590s. These trial records form the core of his book, a fascinating study that captures the religious subculture and political intrigue of Philip II's Spain as vividly as any novel.

Kagan begins by reconstructing the life of the mysterious Lucrecia, the daughter of a humble Madrid legal agent. Using an impressive array of sources, he explores the expectations, social roles, and cultural influences experienced by many women in early modern Spain. Particularly effective is Kagan's description of the rich oral and visual culture, the cadre of "informal teachers" (p. 26), and the omnipresent "word-of-mouth network" (p. 29) of Madrid's neighborhoods and court society that helped to transform a woman of modest means and education into a visionary celebrity.

He then takes us from the streets of Madrid to the world of the imagination. A very helpful discussion of the theory of dreams and their interpretation in Renaissance Spain sets the stage for Lucrecia's dreams themselves. Here Kagan analyzes a unique source for early modern European history, the registers in which the young seer's partisans, particularly her confessor, Alonso de Mendoza, recorded some

415 dreams between 1587 and 1590. These extraordinary dreams, filled with strange messengers, enigmatic prophecies, and terrifying scenes of destruction, are notable for their overtly political content. Their overarching theme was "the loss of Spain"; impending imperial decline and domestic turmoil due to the moral failings of the king, Philip II.

These alarming dreams—and their dreamer—soon became the talk of Madrid. Was Lucrecia one of God's chosen prophets, or an evil and seditious schemer? Did she fabricate these visions, or had she fallen prey to a malevolent demon? Kagan rightly focuses on the interpretations her dreams received in the tense atmosphere of the late 1580s. He skillfully places the case in its political context, situating Lucrecia and the members of her cult among the factions of courtiers and clerics struggling for power in the aftermath of the affair of the notorious traitor Antonio Pérez and the defeat of the Armada (a disaster predicted in the dreams). Well-connected patrons managed to protect the young visionary for two years, but eventually Lucrecia was arrested by the Inquisition on charges of false prophecy and sedition.

Kagan concludes his study by examining the complex trials of Lucrecia, Mendoza, and other defendants, which lasted over five years. He cuts through the byzantine legal process to present an intelligible, even compelling narrative, something that cannot be said of all historians of the Inquisition. Finally, Kagan speculates on Lucrecia's ultimate fate and discusses the meaning of her case for the history of gender roles, the uses of prophecy, and the possibilities of political dissent in Philip II's Spain.

While Kagan's study is exceedingly well documented, it is not entirely unique. It fits squarely into a current trend in Hispanic studies among both historians and literary critics. Many scholars, especially feminists, have been involved in the enterprise of uncovering previously unknown women's lives and voices, and using inquisitorial documents as biographical sources. Surprisingly, Kagan makes no mention of such recent works as Ronald E. Surtz, *The Guitar of God: Gender, Power, and Authority in the Visionary World of Mother Juana de la Cruz (1481–1534)* (1990); Mary Elizabeth Perry, *Gender and Disorder in Early Modern Seville* (1990); Electa Arenal and Stacey Schlau, *Untold Sisters: Hispanic Nuns in Their Own Words* (1989); Milagros Ortega Costa, *Proceso de la Inquisición contra María de Cazalla* (1978); Claire Guilhem, "La Inquisición y la devaluación del verbo femenino," in *Inquisición española: Poder político y control social* (1981); José Luis Sánchez Lora, *Mujeres, conventos y formas de la religiosidad barroca* (1988); or Mary E. Giles, *The Book of Prayer of Sor María of Santo Domingo: A Study and Translation* (1990). And this is hardly a comprehensive list. Placing this work in proper historiographical context in no way detracts from its own merits, but rather distinguishes it as an

important contribution to an exciting field of historical scholarship.

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ANTONIO M. HESPAÑA. *Visperas del Leviatán: Instituciones y poder político, Portugal, siglo XVII*. (Humanidades/Historia.) Madrid: Taurus Humanidades. 1989. Pp. 482.

This book by Antonio M. Hespanha, which first appeared in two volumes under the title *As Vésperas do Leviathan* (1986), makes more accessible a valuable study of early modern Portugal. But press exigencies precluded publication of volume two, which contains extended runs of computer-generated data. Researchers interested in examining those data will thus need to consult the Portuguese edition. Nevertheless, the work under review contains many tables summarizing the results of an impressive effort at demographic quantification.

Of the various documentary sources Hespanha uses, the most important is a two-volume *códice* in Lisbon's Biblioteca da Ajuda, the *Livro das Avaliações de Todos os Offícios do Reyno de Portugal, Anno 1640*. This and other sources yield information that revise our view of Portuguese history. The data reveal, for example, that population growth did not stagnate or decline significantly after the sixteenth-century Golden Age. Instead, it rose modestly, from approximately 1.8 million in 1527 to 1.85 million in 1700. The study also includes data on crown revenues (1527–1680) that confirm previous research suggesting that income increased more from new taxes than from longstanding sources. Moreover, this study gives us our first in-depth picture of landholdings and the administrative apparatus in seventeenth-century Portugal. Of the more than 12,000 officials in the country in 1640, approximately two-thirds held judicial and municipal positions. Crown seigniorial control stood at approximately one-third: 30 percent of holdings, 36 percent of land area, and 42 percent of population.

Using these and other data, the author endeavors to map the contours of power and authority in Portugal. He derives theoretical support from a variety of scholars who have examined the relationship between demography and power, especially Michel Foucault and Claude Raffestin. Hespanha also employs theoretical constructs developed in his earlier writings. The result is a portrait of fragmented power, a mosaic of circumscribed—sometimes competing, sometimes cooperating—political entities.

As Hespanha readily admits, his analysis of power as a function of demography remains tentative, as does his conclusion that documentary sources yield evidence foreshadowing the coming of a Hobbesian "Leviathan" to Portugal. There is certainly room to

question the theoretical scaffolding that Hespanha has erected amid extensive archival research and computer-generated data, but one can only applaud the illuminating role that his book plays in the study of early modern Portuguese history.

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JAMES D. TRACY. *Holland under Habsburg Rule, 1506–1566: The Formation of a Body Politic*. Berkeley and Los Angeles: University of California Press. 1990. Pp. ix, 330. \$45.00.

After James D. Tracy's first study of early sixteenth-century Holland (*A Financial Revolution in the Habsburg Netherlands: "Renten" and "Renteniers" in the County of Holland, 1515–1565* [1985]) appeared, one reader commented that a few more such books would enable us to write a new history of the Dutch Revolt. But who expected Tracy to produce those few books singlehandedly, as this latest installment suggests he might? Once again he guides us with ease and skill through virtual *terra incognita*: the Low Countries before the revolt.

Tracy's quarry this time is the political experience of Holland under Charles V and the early reign of Philip II. His approach and results will interest those who study early modern politics in general or the background of the Dutch Revolt in particular. Holland serves as an example of a little-known intermediate level of political organization, the self-governing province; it was not, however, just another member of this species. Tracy shows through deft comparisons with other areas of Europe that Holland's arrangement was unprecedented: it was a "self-governing province with an integrated structure of authority ruled not only by a prince, but by a consensus among its component parts" (p. 2). This is a history of political institutions in action. Tracy shows the tangible consequences of that structure, which he sums up in the term Holland's "apprenticeship" (p. 6). This might conjure up a scheme of inevitability—that early developments necessarily led to the revolt—but Tracy avoids such a pitfall; rather, we understand the revolt better when we realize that during the preceding decades Holland's basic political apparatus and confidence had already been born.

Comfortably at home in the world of late-medieval and sixteenth-century institutions, terms, and customs, Tracy shows at length the nitty-gritty of Holland's growing political unity. Subtle argument and skillful use of manuscript and printed sources take him far from determinism. For instance, the creature Holland was not so much the product of enlightened officials as a combination of past customs, old circumstances, and new events: part of the province's apparatus even after the revolt had been invented and imposed by the Habsburgs; provincial rule was facil-

itated by small size and convenient waterways; and the towns worked together only when external events almost compelled them to do so. Yet another contribution to unity was the frequent absence of Charles V, which seems as important to the shape of early sixteenth-century Europe as his presence. Tracy illustrates as well how the old Burgundian princes became dependent on the towns, not unlike the method of negotiation outlined by J. Russell Major for France (*The Growth of Representative Government in Early Modern France* [1980]). The essence of Tracy's previous book—that Holland came to take an active role under Charles V in managing the Habsburg's debts through the issuance of low-interest bonds in its own name—is present (chapter 5), but set in the broader context of "apprenticeship," alongside treatments of Holland's growing unity in managing war (chapter 3), commerce (chapter 4), and questions of religion (chapter 6). Experience in each area contributed to a more unified—but not independent—political system and consciousness than had existed in 1500.

My criticisms are of a technical nature. Tracy uses too many untranslated Dutch terms (*lichaem, Informacie, acceptatie, binnenvaart*). He offers an unsatisfactory translation of *renten*, which another reviewer has pointed out may be easily and clearly rendered as "bonds," and though *vroedschap* might mean "wisdom," it is better translated as something like "city fathers." And why follow English usage with some names (Margaret, John) while putting others in the relevant native form (Juana, Willem, Guillaume, Chistiern)? But historians of the polyglot Low Countries could go on forever about such matters, which do not detract from the significance of this book.

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ROBIN BRUCE BARNES. *Prophecy and Gnosis: Apocalypticism in the Wake of the Lutheran Reformation*. Stanford: Stanford University Press. 1988. Pp. viii, 371. \$39.50.

The basic argument of this work is far simpler than is its subject. According to Robin Bruce Barnes, prophecies, the occult, arithmetic mysticism, astrology and magic, and even Rosicrucianism enjoyed great popularity in specifically Lutheran Germany between Luther's death and the early stages of the Thirty Years' War for reasons that had much to do with the nature of Lutheranism itself. A "broadly based Lutheran expectancy [of the Second Coming] contributed strongly to the magical and occult strivings so characteristic of German thought in the decades around 1600" (p. 184). Moreover, whereas other Christian traditions looked to improve the world as it existed, "Lutheranism looked to immediate transformation and liberation from history" (p. 266).

Nonetheless, an equally simple question must follow a close reading of the book: Was it really so? Doubts arise at the beginning of the first substantive

chapter, where the author moves from the observation that after Luther's death "the number of prophetic interpretations and apocalyptic warnings published in Lutheran centers rose rapidly and steadily" directly to a listing of "several causes" that may explain "the desire of Lutherans to expound their eschatological teachings so forcefully" (p. 60). Given that scholars such as Miriam Chrisman have calculated the frequency of publication on these and other themes (however impressionistic even numbers are in the final analysis), it is most discomfiting that nothing of the kind is forthcoming in this work.

The second clue that perhaps it was not really so comes from what the author substitutes for numbers. Readers are told that the rate of publication for such works was "astonishing even by modern standards" (p. 60), that "In the 1550's collections of Luther's prophecies began to appear regularly" (p. 61), that there was a "great proliferation of apocalyptic writings" (p. 63), and that "Lutherans . . . more and more frequently expressed a sense of futility," while "many threw themselves into a renewed search for specific testimonies to the coming Judgment" (p. 71). Argument by assertion continues throughout the volume, as the reader learns that "many Lutheran clergymen" had a "generally positive initial reaction" (p. 222), while a "majority of mystical chiliasts shared a Lutheran background" (p. 227) and their books found "a large reading public" (p. 229). A simple arithmetic summary of titles with appropriate keywords from the marvelous card catalogue at the Herzog August Bibliothek would have been reassuring in the place of all these adjectives and adverbs.

Once suspicions have been raised, it is not easy to suppress the sense that one is standing on sand. Were all the people cited actually "Lutherans," and, if so, what does this label mean? Sebastian Franck, for example, earns the title of "a Lutheran turned independent" (p. 83), one no contemporary observer would have recognized. Johann Sleidan, the great historian of the reign of Charles V, becomes the "historian of the Schmalkaldic League" (p. 108), while "Carion wrote for a Lutheran audience" (p. 145). A certain Eustachius Poyssel appears often (pp. 133, 167–68, 173–74, 197–98), to illustrate "Lutheran" thinking but then is quoted as being for "Schwenckfeld, Osiander, Franck, and others" and against "Luther and Melancthon" (p. 238). In sum, the author by no means demonstrates any connection, whether organic or incidental, between early German Lutheranism and the worlds of at least semi-learned mystical speculation that were so common in the late sixteenth century.

Barnes has nonetheless done well to investigate these worlds and has reported accurately the contents of the texts he has studied. But it must not be forgotten that astrologers and "New Age" mystics still abound. Perhaps trying to explain them by reference

to other movements of the human spirit is to attempt the impossible.

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KRISTIN ELDYSS SORENSON ZAPALAC. *"In His Image and Likeness": Political Iconography and Religious Change in Regensburg, 1500–1600*. Ithaca: Cornell University Press. 1990. Pp. xv, 279. \$29.95.

The best kind of cultural history dictates the crossing of conventional disciplinary boundaries. Hence, when Kristin Eldyss Sorenson Zapalac sets out to write what her subtitle claims to be an account of "Political Iconography and Religious Change in Regensburg, 1500–1600," she freely avails herself of pictorial sources as diverse as book bindings, woodcuts, and frescoes. Her textual sources include wills, civic ordinances, and theological tracts. Like the Regensburgers themselves, she mingles Lutheran doctrine with local politics. The result is a cumulative study of meticulously well-analyzed detail. If the big picture of reformed Regensburg does not emerge fully synthesized after her discreet chapters, the Reformation discourse underlying practical, even formulaic phrases and imagery nonetheless finds its distinctive Regensburg manifestations.

Zapalac's sensitive and persuasive findings are exemplified by her first two chapters on the iconography of justice in the Regensburg Rathaus. Using the local shift from a Last Judgment decoration to an allegory of justice personified among other virtues in the magistrates' chamber, Zapalac pointedly investigates the representation of judgment itself to chart a general shift in Lutheran cities during the sixteenth century. The new creed split off the binding sense of legal oaths to eventual divine justice, or the immanent interconnection between sacred and secular realms. After Luther, salvation no longer depended on civic activities and judges on earth were no longer taken to be the mirrors of eventual divine judgment. Salvation resided solely in the hands of God, now rendered as a merciful father rather than a stern judge, and temporal matters lay squarely in the hands of local authorities. Zapalac's study of wills and ordinances reinforces her visual analysis, displaying how much of the prose of both wills and ordinances relied on a formula of the paternalistic, protective civic authority. Justice depictions at the end of the century feature allegories of virtues, chiefly justice, charity, and prudence, as well as some manifestation of good government, all manifestly stemming from divine sanction. Zapalac's charting of this shift uses a rich variety of sources, including the woodcuts and text of Ulrich Tengler's widely used treatise, *Laienspiegel* (1512), as well as the references to the Last Judgment or God the Father and to the *Rat* as either *Obrigkeit* or *Vater* in local Regensburg wills. Throughout this

argument, her unwillingness to remain content with previous studies, such as Craig Harbison's on Last Judgment images (*The Last Judgment in Sixteenth-Century Northern Europe* [1976]), and her sensitivity to shifts of images and their use, is a model of scholarly rigor and thoroughness.

Of course, many of Zapalac's images, not to mention her Lutheran theological treatises, come from other cities, such as Nuremberg and Wittenberg; the distinctiveness of Regensburg, if any, is not always clear. (Nor are comparisons to other centers—such as Nuremberg's town hall—developed beyond notes or citations. One missed opportunity is the extended comparison of decorations from the Regensburg Rathaus frescoes, figures 39 and 62, with Nuremberg's, published by Matthias Mende.) One suspects that a much larger study has been cut down, especially in the instructive and still copious notes. This focus on Regensburg would be more cogent if fuller comparisons had been used and analyzed throughout. Moreover, significant events in local history—and imagery—such as the extermination of the Jewish ghetto and the subsequent fervent cult of the *Schöne Maria* of Regensburg play no role in Zapalac's consideration of the visual culture of the city and its religious politics. The works of dominant local artists, such as Albrecht Altdorfer (also a Ratsherr) and Michael Ostendorfer, in fostering such local phenomena remain neglected. In short, the local history and imagery of Regensburg get lost in this study, although a later segment focuses on the city's casuistry in rationalizing its continued use of the traditional papal keys on its city heraldry within its Lutheran credo after 1542.

A sophisticated introduction on the Lutheran reversal of Augustinian epistemology concerning images and their abstract referents offers a complex yet persuasive analysis of the reversal of priority between word and image during the Reformation. This section deserves a wide readership, yet it suggests a paradox, inasmuch as the relative Lutheran deemphasis of religious images in practice or the iconoclasm of more radical reformers seems to be at odds with the emphasis on sight over reason in this revisionist account of Luther's reversal of Augustine. Another highlight of this study is a subtle, gender-based analysis of images of both virtues and women heroes (chapter 3). Yet this, too, deserves an extended treatment instead of being limited to a discussion of a local bookbinding decoration (whose juxtapositions of Justice/Lucretia and Venus/Prudence are not explained). Like the rest of this stimulating and perceptive volume, here erudition and research are evident, but the reader is left hungering for a more sustained and synthetic vision of Regensburg as well as gender and politics in Reformation discourse.

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ARTHUR E. IMHOF. *Lebenserwartungen in Deutschland vom 17. bis 19. Jahrhundert*. Assisted by ROLF GEHRMANN et al. (Acta Humaniora.) Weinheim, F.R.G.: VCH. 1990. Pp. 493. DM 148.

This is an unusual book. More than half of its considerable length consists of hundreds of tables, few of which are mentioned in the text. The pieces of text, by five authors, have little connection to each other. Only a handful of pages contain analyses of the mortality data that fill the tables. Arthur E. Imhof's introductory essays, printed in both English and German, explicitly renounce any intention of producing a demographic interpretation of the masses of data collected and analyzed by his research team at the Free University in Berlin. He offers rather a research tool, an extensive data base, for others to use.

The production of this computerized data base is a remarkable achievement, which, if taken full advantage of, will influence historical demographic research for years to come. Over 135,000 individual life spans are recorded for seven small regions in western Germany between 1740 and 1860. The groundbreaking work of John Knodel in *Demographic Behavior in the Past: A Study of Fourteen German Village Populations in the Eighteenth and Nineteenth Centuries* (1988) might now be considerably widened. Regional or confessional differences in life expectancy, the influence of local or national economic fluctuations, and urban-rural comparisons of mortality can all be pursued in much greater detail. Imhof rightly claims that the value of these data is not limited to Germany or even to Europe; because the data cover the early stages of declining mortality, the experience of the German population will also be of interest to demographers examining Third World countries.

Evidence for thousands of individual lives was derived from a peculiarly German source, the *Ortssippenbücher*, genealogical compilations of family histories for local populations. These rest in turn on parish registers, sharing both their accuracy and limitations (for example, difficulties in dealing with migration). The accumulated labors of generations of parish priests and genealogists enabled the Berlin demographers to amass an enormous data base.

These data, in computerized form, are now available from the Zentralarchiv für Empirische Sozialforschung in Cologne. For all data users, this book is a necessary companion: full methodological discussions, detailed descriptions of the source regions, and a printout of the data themselves. Readers, however, need to exercise some caution. The various authors are undecided whether there are six or seven regions; data for Hamburg are included in the tables but Hamburg is omitted from the regional chapters. Column headings in the tables refer to periods by noting only the first year. Small numbers of cases are transformed into decimals with four places, as if any

demographic source could possibly be accurate to hundredths of a percent.

Outside of a small audience, however, the book is unlikely to stir much interest. The endless pages of tables are unreadable. Descriptions of the regions, written by Ines Klope and taking up half of the text, will be useful mainly to local specialists. Nothing in these descriptions is explicitly related to the issues of life expectancy or mortality, although the consequences of wars, epidemics, bad harvests, and other economic dislocations might well show up in the tables. And who wants to read the methodological discourse by Rolf Gehrman and Maureen Roycroft without the payoff of data analysis? That leaves only Imhof's preface and introduction to appeal to a broader readership, and here his decision to eschew analysis of the data has robbed this book of any wider purpose. The potential for interesting results is evident in the single bit of analysis Imhof does provide, a discussion of changing life expectancies for women. Imhof argues that the pattern of excess female mortality in the nineteenth century has since been reversed. Because women have asserted control over their own bodies, by limiting the number of births, they have reduced a significant cause of female mortality. Imhof's justification for leaving further analysis to others—he claims that the multidisciplinary nature of mortality research would make any interpretation questionable or one-sided (p. 19)—sounds peculiarly pessimistic. If historical demographers cannot interpret their own data, who can?

Although the overall results for life expectancy at birth and infant and child mortality up to age five are never mentioned in the text, the graphs on pp. 199–200 and pp. 208–09 show close agreement with Knodel's data for 1740–1850 (pp. 42–43, 59). Despite Imhof's assertion that historical demography can now only be properly pursued in research teams (p. 9), Knodel offered a more complete explanation of data sources and methods, as well as a full interpretive analysis. Imhof's claim (p. 9) that this project supersedes Knodel's work by expanding the data base thus remains to be demonstrated.

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NORBERT FINZSCH. *Obrigkeit und Unterschichten: Zur Geschichte der rheinischen Unterschichten gegen Ende des 18. und zu Beginn des 19. Jahrhunderts*. Stuttgart: Franz Steiner. 1990. Pp. 335. DM 80.

For those who—when eighteenth-century paupers, beggars, smugglers, and robbers come to mind—are uncertain how to categorize them sociologically, let alone imagine them with human faces, there is much to be gained from reading Norbert Finzsch's book. For those who are inclined, following Max Weber, Norbert Elias, or Michel Foucault, to think that the Age of Absolutism and Enlightenment made a deci-

sive contribution to the social disciplining of the common people, Finzsch's argument will pose a challenge.

The *Unterschichten*, or "under-classes," that Finzsch investigates were mostly inhabitants of the city of Cologne, which, together with the rest of the Rhineland, in 1794 fell under French occupation, before being annexed by Prussia in 1815. Finzsch tests the utility of the under-class concept by a sophisticated quantitative analysis of rich archival materials, smoothly presented in narrative form. He targets four groups: the resident poor (*Hausarme*), "sedentary" and "non-deviant"; beggars, "non-sedentary" but, in the Old Regime at least, also non-deviant; smugglers, settled but deviant; and robbers (mostly gang-organized), who were both non-sedentary and deviant.

Under the Old Regime, the resident poor (whose numbers in bad years could swell to a fourth of the city's population of roughly 40,000) lived "in social harmony" (p. 281) with their surroundings, claiming and receiving charitable support in Christian compensation for their advanced age, widowhood with dependent children, or under-employed, impoverished artisan status. The numerous beggars labored under a more grudging toleration, shielded by Catholic teachings and the advanced age or ill-health that were their characteristics. Many had once plied a trade, or still tried to do so.

Before the French occupation, repressive policies toward the poor were confined to efforts to banish interloping mendicants and to the building of a poorhouse and workhouse for sturdy beggars. Neither of these institutions applied more than minimal physical punishment. Both fed their inmates well. The workhouse came to function mainly as a residence for castoff relatives, abandoned children, and sundry social offenders. Prisons were not built, poverty was not medicalized.

After 1794 the French authorities, having secularized church assets previously devoted to poor relief, subjected the resident poor to a bureaucratized and morally censorious dole, from which those who were young and fit to work were excluded. Efforts to outlaw begging and to confine able-bodied indigents to newly built workhouses were partial successes. After 1815, the Prussian regime further criminalized mendicancy, while installing a decentralized system of poor-relief overseers (*Armenväter*) to administer the dole to the deserving poor.

Smuggling and organized robbery soared at the turn of the new century. The Rhineland's incorporation into the French economic sphere cut Cologne off from its right-bank hinterland and made smuggling into a profitable business. Many ordinary working people engaged in it without loss to their social reputations. Organized bands of highwaymen and thieves, such as the famous Schinderhannes gang or the formidable "big Netherlands gang" (*Grosse Niederländische Bande*), flourished amid the multitudinous

army desertions of these years. But the local French administration, with its (hitherto unknown) bureaucratic centralism, prisons, and police efficiency, ruthlessly stamped them out. Whether they expressed any social rebelliousness is doubtful. Schinderhannes, seeking to keep the German villagers on his side, selected his victims from among the Jews. Finzsch argues plausibly, although without tracing out the empirical linkages, that popular resistance to the bureaucratization, criminalization, and medicalization of poverty eventually found nineteenth-century expression in socialism.

A virtue of Finzsch's study is its demonstration that the "under-classes," like the condition of poverty surrounding them, are social and scientific constructions. A question his book raises but does not answer is whether the material deprivation the "non-deviant" (and non-institutionalized) poor suffered under the Old Regime was so painful as to render their social integration merely formal.

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DANIEL MORAN. *Toward the Century of Words: Johann Cotta and the Politics of the Public Realm in Germany, 1795–1832*. Berkeley and Los Angeles: University of California Press. 1990. Pp. vii, 304. \$40.00.

The title and opening epigraph of this book are taken from one of Klemens von Metternich's gloomy prophecies about what the nineteenth century was about to inflict on his aristocratic kind and his regime. The book ends with Daniel Moran's gloomy prophecy about what the twentieth century would do to Johann Cotta's kind and their regime, when "*Unparteilichkeit*, on which personal culture and public authority were equally dependent, would become just another form of service to the state" (p. 276). Between these systems falls the medium, an ideational realm rather like the ether, through which the intellectual product of a cultural elite was to be transmitted to a receptor public. Cotta's aim was "to define, and institutionalize, a realm of action beyond the control of the state, but short of opposition to it—a 'public' realm in which intellectual independence and political loyalty would equally well be served" (p. 18).

To assign so theoretical and teleological a role to the energetic and pragmatic publisher of the great daily *Allgemeine Zeitung* may seem burdensome, and it may seem best initially to treat Moran's intelligent and judicious book as a political study, which is essentially how he presents it, or better perhaps, as a study of political intentions. That way, we are less troubled by the incongruity between Cotta's announced goals of political reform and his embarrassing subservience to the requirements of restoration politicians such as Metternich and Friedrich von Gentz. Besides, we are told too little about Cotta's personal life and experi-

ence and his education to judge how he arrived at the larger goals and self-definition he assumes here. The use of the biographical form is "not to recapture the full complexity of Cotta's experience but rather to use his experience to illuminate his age" (p. 19). Cotta himself functions here as a medium.

Cotta as such often becomes invisible for many pages while other notables of his age perform before our eyes. Nevertheless, the most light shines on the business in which Cotta was engaged. We are reminded that the main legal problem of the business of publishing was not censorship but piracy, and that publishers could hardly claim official protection against piracy without invoking censorship; moreover, "Cotta always assumed the press could serve the public best if it could also be made useful to government" (p. 152). Indeed, it often appears that state patronage and not the marketplace either of information or of commerce was the economic as well as political cornerstone of Cotta's journalism, a condition that the Biedermeier publisher found quite natural and acceptable. Political conformity in the medium was self-evidently a matter of business.

Moran poses Cotta and his stable of publications theoretically as media of passage between marketplace and ideas, a construction much in use among present students of communication and opinion. Some other readers may be forcibly reminded of Immanuel Kant's famous distinction between public and private spheres of communication and opinion, where, in his essays on the Enlightenment and on the academic disciplines, the philosopher managed simultaneously to join full freedom of enlightened public opinion with, at the same breath, a subordination of useful knowledge to the practical interests and administration of the state. The philosopher's injunctions too might be useful and possibly more generous terms for appreciating Cotta's deference to police authority over political opinion while at the same time proclaiming an enlightened public's right to know; and often Cotta seems almost to be trying to act out Kant's paradoxical program. More interesting still might be a historical comparison, an examination of interaction or transition between these two kinds of control, by police, and by the market, on the mediation of ideas.

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JAMES Q. WHITMAN. *The Legacy of Roman Law in the German Romantic Era: Historical Vision and Legal Change*. Princeton: Princeton University Press. 1990. Pp. xvi, 281. \$39.50.

By the author's own confession much of modern legal history consists of unrealized hopes and miscalculated programs. Such was the fate of the history of Roman law in Germany. Yet legal history can also provide an invaluable clue to the political and social conditions of

a period. James Q. Whitman presents us with a fine example of such a genre in which he combines legal, intellectual, and cultural history in a most enlightening way. Based on extensive reading of printed sources, the superbly crafted and well-argued book is more far-reaching than its title suggests.

As the central piece of his work, the author attempts to show how the Roman law professoriate became a political force in post-Napoleonic Germany. He centers his account primarily around Friedrich Karl Savigny and his "Historical School," while bringing in other prominent legal scholars such as Anton Friedrich Justus Thibaut and G. F. Puchta. The issue around which Whitman organized his thesis is the so-called *Agrarfrage*, that is, the emancipation of the peasantry in various German states from feudal burdens. Savigny and his followers believed that end could be achieved through the application of certain Roman law principles, such as *servitudes* and prescription, and by the use of *Aktenversendung*, the practice of referring cases to the law faculty in a particular German territorial university for a decision. In short, the reasoning was that judicial decisions should be made by panels of legal scholars rather than by precedent, statute, or judge. By these means, therefore, a social revolution was to be carried through. Eventually the efforts of this Historical School would be defeated. Whitman attributes this failure after the 1850s to the twin challenge of, first, what he calls the attack of the romantic Germanist lawyers who accused their Romanist colleagues of being cryptoreactionaries reaching back to the alien law of Antonine Rome and, second, materialism, formed from an amalgamation of science and industry and characterized by ostentatious practicality. The success of materialism began with the drawing up of the commercial code and was finalized by the appearance of the *Bürgerliches Gesetzbuch* in 1900.

But Whitman only uses this episode in German legal history to give us a panoramic view of the vicissitudes of Roman law and Romanist lawyers in the Holy Roman Empire since the Reformation era, stressing the role of Melancthon. In the eighteenth century the rise of the absolute states propped up by natural law theory led to codification and, with it, a declining role for Roman law and the legal faculties in the judicial process. Only with the revival of *Reichspatriotismus* in the 1790s came a reversal, which Savigny later sought to exploit. This part takes up half of the book, a veritable tour de force.

Admirable as the work is, some minor clarifications are warranted. While properly making a clear distinction between Roman law and the *ius commune* in his early chapters, Whitman does at times leave the reader confused as to their proper relationships. His abrupt break between the legal developments of the sixteenth and eighteenth centuries could have been bridged by looking at the evolution of German public law in the seventeenth century, which, while retaining

the outward format of Roman law, was based solely on German legal tradition.

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CHRISTOPH HAUSER. *Anfänge bürgerlicher Organisation: Philhellenismus und Frühliberalismus in Südwestdeutschland*. (Kritische Studien zur Geschichtswissenschaft, number 87.) Göttingen: Vandenhoeck und Ruprecht. 1990. Pp. 378. DM 74.

In the 1820s, enthusiasm for the Greeks in their struggle for national independence against the Turks swept Germany and much of Europe. Children played Greeks against Turks; their elders dressed up in Greek costumes; men shot at targets with Greek themes. Christoph Hauser investigates this phenomenon to throw light on a dim period of Germany's past. He argues convincingly that the philhellenic movement initiated organized, civic participation in public debate on a broad basis, especially in the southwest states of the German Confederation. The German love affair for things Greek is often noted and the relevance of the Greek war of national liberation to German politics could be guessed, but no one has studied in detail the hundreds of clubs that formed after 1821 to rally political backing for the Greeks, raise funds for their struggle, and recruit and finance nearly 450 volunteers to fight on their behalf (between 270 and 300 eventually went to Greece). Hauser has left no source unused, having examined dozens of newspapers and journals, gone through philhellenic archives throughout southwest Germany and Switzerland, explored state records relating to the clubs, and read diaries, letters, memoirs, and publications by promoters of the Greeks. A full third of the book is given over to notes, sources, thirty-eight maps, eight tables, and four graphs.

Although philhellenism was a pan-German movement, Hauser restricts his investigation to southwest Germany, where publicly organized support for the Greeks met the least repression. Politically the most progressive region of Germany, this was where ties between the movement and the political opposition in the state legislatures most easily flourished. He relates advocacy for the Greeks to domestic political issues and shows how the clubs' form of organization (federatively linked, with each club governed democratically by majority rule) as well as their ideology challenged existing authoritarian, monarchical political structures.

This study is no mere narrative or catalog of philhellenic activity. Hauser is intent on milking every last drop of analytical substance he can, even when his sources are scanty and do not much bolster statistical inquiry. He scrutinized the background of key club members, some 1,870 active organizers. On the whole, he found they stemmed from urban groups such as administrative and judicial officials, clerics,

and educators, along with some business and commercial classes. His claim that philhellenism was generally representative of the population is better buttressed by his evidence that financial contributions to the cause also came from women, lower urban classes, and peasants. His finding that a quarter of a million Germans (7 percent of southwest Germany's population) were involved in one way or another amply demonstrates that organized philhellenism helped extend political participation beyond the legal electorate of the 1820s and prepared the stage for the surge of political activity in 1830 and afterward.

In thumbnail sketches of the histories of Württemberg, the Grand Duchy of Hesse, Baden, and Rhenish Bavaria (Palatinate), Hauser attempts to embed the philhellenic clubs in the region's social, economic, and political background. This is useful in setting the framework for the movement's activities, but is the book's weakest part in suggesting new conclusions.

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JÜRGEN KOCKA. *Weder Stand noch Klasse: Unterschichten um 1800*. (Geschichte der Arbeiter und der Arbeiterbewegung in Deutschland seit dem Ende des 18. Jahrhunderts.) Bonn: J. H. W. Dietz. 1990. Pp. 320. DM 48.

JÜRGEN KOCKA. *Arbeitsverhältnisse und Arbeiterexistenzen: Grundlagen der Klassenbildung im 19. Jahrhundert*. (Geschichte der Arbeiter und der Arbeiterbewegung in Deutschland seit dem Ende des 18. Jahrhunderts.) Bonn: J. H. W. Dietz. 1990. Pp. xiii, 722. DM 98.

These two substantial volumes represent the first half of Jürgen Kocka's projected four-part study of the formation of the German working class and the founding of the German workers' movement in the first three quarters of the nineteenth century. *Weder Stand noch Klasse* examines the extremely diverse lower strata of society at the end of the eighteenth century, while *Arbeitsverhältnisse und Arbeiterexistenzen* explores the uneven development of wage labor among that segment in the nineteenth century. Volume 3 will continue with an analysis of working-people's lives, cultures, and protest, while the concluding book will focus on workers' economic and political organizations. Kocka's study is, in turn, part of a multivolume history of the German working class in the nineteenth and twentieth centuries to which Klaus Tenfelde, Gerhard A. Ritter, and Heinrich August Winkler are contributing.

Eschewing any simple, reductionist, or teleological concept of class, Kocka sets out to explore the complex of economic, political, and cultural factors that led some, but not all, members of the lower strata to make class their primary identification and a class-based movement their main form of economic and

political action. To an extraordinary degree he pays attention to the varied, uneven, and contingent character of the processes he studies. But he studies these processes primarily from a structural perspective, rather than an experiential one. Kocka is critical of both E. P. Thompson's culturalist approach and German *Alltagsgeschichte*, which emphasize workers' perceived experience, the culture of work and everyday life, and the isolation and autonomy of those at the bottom of society. Instead, he gives priority to examining how broad structural changes in the state and the law, in economy and population, and in property and authority structure the life chances, obligations, and forms of dependence of the lower orders. He insists on constantly connecting history from below with history from above. And he resolutely refuses to romanticize the preindustrial world or condemn proletarianization as an entirely negative process. On the contrary, Kocka acknowledges the cost of modernization but argues for its emancipatory aspects, at least in the long run.

Weder Stand noch Klasse presents a synchronic picture of late-eighteenth-century Germany, where the corporate order had begun eroding but a working class had not yet begun to form. The book's subject is the lower strata from which a working class was eventually to emerge. Comprising the one-half to two-thirds of society who were threatened with poverty, the lower strata were not an estate of the corporate order, for their sources of subsistence, status, and the forms of dependence in which they were enmeshed were too diverse. But nor were they a class, for they by and large did not engage in wage labor, were subjected to traditional forms of authority, and had such diverse situations and interests that they could not develop a shared identity or act collectively. Kocka explores the varied forms of authority and obedience that were the principal structuring elements of the corporate order and examines the precapitalist economy that characterized the multiplicity of German states. He details the multiple types of poverty, inequality, and insecurity in town and country, among women and men, as farmers or farm servants, as masters or journeymen.

Kocka's concluding chapters on protest and conflict draw interesting comparisons with eighteenth-century England. Whereas the English laboring poor protested against the encroachments of capitalism and the market, the German lower strata protested against feudal dues and authoritarian structures. English protesters formed crowds of consumers, while German protesters acted in discrete groups as subjects of traditional authorities or as producers in guilds. And he correctly concludes that, despite growing tensions and actual protest, a revolution on the French model was not on the historical agenda, for economic backwardness, political fragmentation and absolutism, religious divisions, and cultural traditions all worked to curb and disperse protest more effectively than in western Europe.

Volume 2 explores working-class formation in the nineteenth century, an era of multiple and fundamental transitions in the German economy, law, state, politics, and in forms of authority. His focus is on work relations, technical and social, and on the uneven emergence of wage labor that was an integral part of what he calls the decorporatization of German society and the emergence of capitalism and industrialization. Work, insists Kocka, was the main determinant of material conditions, ways of life, and identities for the lower strata. The culture of everyday life, gender relations, and consumption were of secondary importance in shaping what it meant to be a worker.

Kocka's survey of the spread of wage labor in the nineteenth century covers not only the usual groups (artisans, miners, and factory workers) but the more neglected ones as well (servants, small farmers and farm laborers, and railroad construction workers). In rich detail he explores the great diversity of experiences both within different groups and among them. Servants, rural laborers, and railroad workers remained marginal to the emerging working class either because wage-labor relations were at best partially instituted or because individuals worked in them for only a relatively brief period of their lives. Artisans were a more significant component of the emerging working class, but unlike in western Europe it was only journeymen who identified as workers while masters clung to their corporate identities or became small capitalists. In Germany, unlike in France or Britain, however, the core of the new working class was formed by workers in centralized factories, whether mechanized or not, for they experienced wage labor in its purest form, became more economically and socially homogeneous than other groups, and developed new identities as workers most strongly.

Kocka's conclusions are not startlingly new—he himself has argued them elsewhere in more summary form—but the extraordinary detail with which they are supported and the splendidly rich footnotes make these volumes extremely valuable. But they are not the last word. Although Kocka acknowledges that workers' subjective experience must be studied, he rarely does so. One understands how work structures changed, for example, but not how workers experienced those changes or how work cultures were transformed. Women are included throughout both volumes, but the multiple ways in which gender structures class, in which class is always experienced differently by women and men, is ignored. The organization of this multivolume project not only gives primacy to the world of work but sharply demarcates it from home and community, from culture and consumption. Even Kocka admits that the dividing lines are blurred and shifting. As he turns from work to family, culture, and politics in subsequent volumes, one hopes that these problems will be addressed and that the complex interconnections

among the constituent elements of class will be fully explored.

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OTTO PFLANZE. *Bismarck and the Development of Germany*. Volume 1, *The Period of Unification, 1815–1871* (2d rev. ed.); volume 2, *The Period of Consolidation, 1871–1880*; volume 3, *The Period of Fortification, 1880–1898*. Princeton: Princeton University Press. 1990. Pp. xxx, 518; xvii, 554; ix, 474. \$95.00 the set.

In 1963, Otto Pflanze published a book on Otto von Bismarck and the development of Germany in the period 1815–71. It rapidly became a standard work. Since then, an already enormous historiography has continued to grow, and the larger field of German history in the period has been continually reshaped. The last decade has seen major scholarly biographies by Lothar Gall and Ernst Engelberg, a shorter study by Andreas Hillgruber, works on neglected themes (such as Manfred Hank's book on Bismarck in the 1890s), and many popular biographies. This has played its part in delaying the completion of Pflanze's study. Now, nearly thirty years later, he has given us a revised version of his original work and two further volumes that complete an imposing trilogy. In quality as well as size it is a major work.

One of Pflanze's greatest achievements is the depiction of his central character. He writes very well not only on Bismarck's political and moral values but also on his moods, appetites, taste, family life, business affairs, and health (he is particularly good on the last two). Pflanze is generous with direct quotation, thoughtful but undogmatic on the subject of personal psychology. He presents an unvarnished, often damning picture of an imperious and bullying personality, yet never lacks sympathy. These books successfully show the connections between the public and private man, while the scale of the work allows us to see in illuminating detail the shape of a life divided between Berlin, rural estates, and spa towns.

But this is more than a biography. It is a life-and-times work that ranges widely in chronology and subject matter, from the Prussian state in the seventeenth century to Bismarck legends in the twentieth, from the ambitions of the European great powers to social and religious divisions in newly unified Germany. Pflanze is self-consciously rounded and judicious in approach. He is keen to deny the primacy of either domestic or foreign policy considerations in the making and consolidation of the new Germany, just as he is concerned to plot the interaction between the man and his historical environment. Pflanze has sensible things to say about the tendency of some modern writers to replace the genius of Bismarck hagiography with a manipulative evil genius. His own Bismarck has a good deal in common (perhaps more than the author accepts) with what he calls the "Bis-

marck-the-diminished-genius" view of Gall, although the thrust of his argument is less deliberately demythologizing. Like Gall, Pflanze makes good use of Bismarck's own conception of the "stream of time" that carries historical actors along. And he provides a similarly convincing account of the way in which Bismarck lived politically from hand to mouth in his later years in power. Overall, Pflanze offers a split verdict. The statesman was successful beyond the triumphs of 1864–71, largely maintaining his touch as he steered the "satiated" German state through the troubled international waters of the following two decades. Domestically, however, he ran out of options in attempting to preserve the Prussian-authoritarian features of the new Reich through a combination of carrot and stick. Pflanze has much that is critical to say about the Bismarckian legacy of repression (Catholics, national minorities, socialists) and the cowing of politically independent voices.

This broad picture may lack surprises, but the virtues of this book do not lie in dramatic new findings or theses, rather in the well-informed assurance with which the mountainous material is handled. Pflanze has read prodigiously and organized carefully. His ability to dovetail many themes is particularly evident in volume 2, the longest and perhaps the best, devoted to the crucial 1870s. Pflanze writes expertly in all three volumes on international relations, but he also shows a deft touch in recounting political maneuvers, the treatment of non-German minorities (French and Danish, as well as Polish), and the development of social policy and taxation issues. The value of his research in the archives of former East Germany is evident here. At a time when Anglo-Saxon historians (John Röhl, Isabel Hull, Katharine Lerman) are exploring the neglected machinery of government in Wilhelmine Germany, Pflanze casts valuable light on "the valves and sluices of power" (vol. 1, p. 48) in the preceding decades.

These volumes provide a valuable work of reference on countless issues. There is, for example, an admirably comprehensive and differentiated account of what German liberals sought and gained (as well as what they sought and failed to gain) in the 1860s and 1870s. On another key issue, Pflanze gives no fewer than ten reasons for the return to protective tariffs in 1879, with an indication of their weighting. He also looks up from the daily political struggles to float some valuable general ideas. Pflanze suggests that the Bismarckian "revolution from above" was the last in a line that stretched back to the seventeenth century, and he offers a tantalizing throwaway observation about the "disconnectedness" of foreign and domestic policy making. The ruminative Pflanze, seen to good effect in the concluding reflections, makes one wish for more of this in the text. Imperial Germany has been a combative field of late. Historians have argued about the economics of unification, Bismarck as Bonapartist or "white revolutionary," and the pursuit of foreign policy for domestic purposes. Pflanze gener-

ally expresses skepticism about such readings, but his new evidence often leads to conclusions that are at least compatible with them. As he frequently demonstrates, the "abstract forces" whose invocation he deplores need not remain abstract in the hands of a skilled practitioner. Many historians neglect to practice what they preach. Here is one who neglects to preach what he practices.

There are inevitably some weaknesses in a work of this scale. The years up to 1858 receive just 100 pages out of 1,500, which is arguably too few given that they made up more than half Bismarck's life and included the formative experience of 1848. Pflanze underplays the strength of nationalism in the 1860s to a degree that is misleading: the "few thousand" members of the *Nationalverein* (vol. 1, p. 139) were actually 25,000 highly influential men. Pflanze has very little to say about elections, even vital ones like 1890, beyond giving brief summaries of the results, and he shows surprisingly little interest in the forms, rituals, and functions of political life in Germany at a time when so much was changing (the decline of "notable politics," the advent of pressure groups and professional politicians, changing patterns of parliamentary business). The author's touch is least sure outside the sphere of politics. Although Pflanze interweaves the personal and political so well, his sections on demographic or economic developments seem less well integrated into the text. He reproduces arguments about the so-called "feudalization" of the German middle classes that have been abandoned by most of those who used to forward them. And there are some worrying translations of terms that describe social classes: intelligentsia for *Bildungsbürgertum*, middle class for *Mittelstand*, lower over-stratum for *Oberschicht*.

But the principal response to this book must be admiration for the decades of assiduous scholarship it represents. The first edition of volume 1 from 1963 was more pointed than its successors (compare "The Bismarck Problem" there with the "Introduction" here); its subsections were shorter, the language was more direct (the "nasty" of 1963 has become the "uncomplimentary" of 1990). The present trilogy is a more serene work. Comprehensive, confidently constructed, and commandingly written, it is the splendid summation of a lifetime's work.

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THOMAS NIPPERDEY. *Deutsche Geschichte 1866–1918*. Volume 1, *Arbeitswelt und Bürgergeist*. Munich: C. H. Beck. 1990. Pp. 885. DM 78.

Thomas Nipperdey is the great German exponent of the Rankean ideal that every historical epoch be judged on its own terms. His survey of the German empire purports to rescue this epoch from the hands

of those who have judged it in the light of a later era—as the point of departure or a way-station along a German *Sonderweg* that led to the Third Reich. Nipperdey aspires to tell the whole story, not only to draw a more balanced judgment but also to provide a history that “comprehends the totality of the life-worlds” that made up the *Kaiserreich* (p. 837). It is an enormous undertaking, which bursts the bounds of a single volume; a second, to cover politics, will follow. The present volume is nonetheless vast in scope, for it comprehends the demographic, economic, social, and cultural structures within which politics took place. The result is a wonderful book, an achievement remarkable for its breath-taking scholarship and erudition, as well as the range of its coverage. It is simply the broadest and richest survey of the German empire since the work of Karl Lamprecht.

The volume is like a grand impressionist landscape. The brush strokes are the carefully balanced sketches and synopses in which the author portrays the myriad particulars of his panorama—from birth control to banking, from art history to industrial technology, and from poor relief to girls' schools. The sections that treat daily life and confession are perhaps the most stimulating. The portraits are all finely crafted in a prose style that is easy, if often mannered. The volume abounds in statistics as well as in strictures against overstatement, exaggeration, and prejudice—all directed against the utopians, dogmatists, and liberals (p. 218) who, in their fervor to criticize the empire's institutions and elite groups, have ignored the historian's obligation to seek what the author calls the “correct proportions” (p. 558). Nipperdey is by no means blind to the “shadowy contours” of the *Kaiserreich*, to which he devotes a brief chapter at the end; but the emphasis in his survey falls consistently on the positive, on social and cultural progress, the spread of prosperity, education, leisure, social legislation, and the gradual abatement of social and confessional conflict.

A broad survey like this is necessarily an exercise in classification. The central problems with which it must contend are aesthetic, in the sense that they relate to the balance, elegance, economy, and heuristic cogency of the categories employed to impose form on the particulars of the past, for these particulars cannot (with due respect to Ranke) order themselves. The explanatory power of Nipperdey's categories will face its main test in the political analysis reserved for the next volume. In the present volume, however, several problems blemish the landscape enough to deserve mention.

Although they have defined or located the processes in different fashion (depending on their origins in Berlin or Bielefeld), the “critical” analyses of the *Kaiserreich*—the targets of Nipperdey's strictures—have emphasized the centrality of class tension in the dynamics that led to the catastrophes of the twentieth century. In Nipperdey's survey, class is a ubiquitous category. It is the primary theme of sec-

tions on “entrepreneurs,” “the artisanate,” “workers,” and the “new *Mittelstand* and the bourgeoisie,” as well as a brief chapter devoted to “class society.” In addition, the problem of class lies athwart at least two dozen other sections, from “sexuality” to “Jews”; it intrudes most persistently into the chapter on daily life and requires the author's repeated reminders (pp. 127, 132, 164, 167, 176, 183, 186) that the analysis of health, clothing, leisure, sport, and the experience of nature cannot be divorced from reference to class. The repetition is not merely an aesthetic problem; it has interpretive consequences. The organizational clutter diffuses the centrality of class—in the name of analytical balance perhaps, although the anemic treatment of other important subjects suggests a more basic ambivalence about the role of class conflict in the history of the German empire. Passages in the volume are weak on the nobility (which merits not even a section of its own), class tension in the countryside, the feudalization of the bourgeoisie, criminality, and social strain within German Catholicism.

Another central category is likewise marred. The principal theme that runs throughout the volume is the growing domination of the values, life-styles, and *mentalités* of the Protestant middle class, a phenomenon that the author calls *Verbürgerlichung* or, with less ease, the establishment of bourgeois “hegemony” (pp. 394, 756). He does not, however, regard this process as problematic; it was, he writes with respect to the workers, “quite normal” (p. 315). His judgment rests on an elastic definition of “culture.” In the early phases of the survey, which deal with social groups, he defines culture broadly as a self-reflexive, interpretive “process” (p. 383), and he writes of a multiplicity of cultures, of “social and moral milieus” (p. 169) that were specific to class, confession, region, city, and countryside. By the end of the volume, however, in a long celebration of German culture that is altogether out of proportion to the rest of the landscape, Nipperdey's definition has lost its breadth, dynamism, and its multiple referents; the subordinate cultures have simply disappeared from view. Culture has become instead the familiar canon of art, literature, music, and academic scholarship, the “high culture” of the urban Protestant educated middle class. Nipperdey's sympathy for this culture is as little concealed as his own immense grounding in it.

The survey is not as free of *Thesen* as the author professes (p. 838). Nor is this review as balanced as it ought to be, for it lacks the space to do justice to the imposing virtues that make this book such a splendid accomplishment. One can only hope that its successor appears soon.

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ANNETTE DREES. *Die Ärzte auf dem Weg zu Prestige und Wohlstand: Sozialgeschichte der württembergischen Ärzte im*

19. *Jahrhundert*. (Studien zur Geschichte des Alltags, number 9.) Münster: F. Coppenrath. 1988. Pp. 360. DM 44.

To the reader with good antennae, a book printed with a glossy, full-color cover (with a painting of a nineteenth-century doctor placing his ear on the naked back of a woman), in the series "Studies in the History of Everyday Life," with a title "Physicians on the Road to Prestige and Prosperity," would signal its message well enough. The agenda of *Alltagsgeschichte* is to return power to the people and to call into question the claims to prestige and prosperity of certain social groups in modern German society. In this case the target is physicians, the key substitutes for the vanished feudal aristocracy (or, in the case of Württemberg, not noted for its powerful feudal aristocracy, stand-in for Rhine-Ruhr industrial barons and Prussian bureaucrats).

But it would be a mistake to judge this book by its cover. This is a very serious study of the medical profession in Württemberg, based on archival research and reproducing statistical material collected by Annette Drees with immense effort. It follows the recent trend in German social history of embracing the concept of professionalization, devoting the lion's share of space to the liberalization and growth of the market for medical services and the resulting "explosion" of physicians' prosperity (Drees's term [p. 209]) and rise in their social prestige.

The unique contribution of Drees's research—as well as the basis of her argument—lies in using Württemberg's family property inventories at marriage and death, a tax registry that only ceased with the advent of the German Civil Code at the turn of the twentieth century. She is convincingly able to establish that at least the urban physicians in that German state possessed, in the mean, more property at their demise in 1871–1900 than in the period 1800–71. The data she has uncovered are fascinating and important. Her generalizations about the history of medicine and the professionalization of doctors in Württemberg comport with the findings of recent sociological and historical investigations.

Drees is too good a historian to make unquantified arguments, and sometimes she seems to contradict her own statements. Given the recent claim of the eminent sociologist of medicine Eliot Freidson (whom Drees frequently cites) that the sociology of professions is a "shambles" ("Are Professions Necessary?" in T. L. Haskell, ed., *The Authority of Experts* [1984], p. 5), such contradictory statements should be counted as forthright intellectual honesty rather than undisciplined confusion.

Drees's central proposal is that Württemberg physicians achieved prosperity in the last third of the nineteenth century through marriage ties to the new industrial *Besitzbürgertum* rather than with the old commercial *Mittelstand*, as in the period before 1871. The ability to marry money rested, in turn, on the

social prestige of doctors, according to Drees. The least important reason for this prestige was the immense alleged contribution of German medicine to the public welfare. Physicians were the beneficiaries, not the instigators, of state health and welfare policies, scientific medical research, and higher education and qualification standards.

In bypassing the (probably unanswerable) question of physicians' professional earnings, Drees refocuses the question of success or failure in the "professionalization project" (M. S. Larson, *The Rise of Professionalism: A Sociological Analysis* [1977]) on property. This is an interesting novelty in the history of German professions and testifies to the firm place of Pierre Bourdieu's theories (*The Inheritors: French Students and Their Relation to Culture* [1979]) about cultural capital in current German social-historical thinking.

The weaknesses of the argument are many and familiar to social historians of the nineteenth century. To what degree did the *Besitzbürgertum* grow as a social group, and what difference did it make to the prosperity of physicians? How does one compare the prosperity of an industrial to a preindustrial middle class? If who one married was more important than one's income, what difference does it make to professionalization theories? Drees's supporting contemporary literary evidence about the primacy of a good marriage over a good practice is often ambiguous and could in many cases be cited to support a contrary argument. In fact, much of what Drees argues could easily be turned around to support an argument of incipient deprofessionalization or failed professionalization in Württemberg. (My own investigations indicate a certain lag effect there, up to World War I.) The use of the measure of income from professional practice remains critical to tracking the professionalization of modern societies. The absence of size comparisons between the pre and post-1871 middle class in general also reduces the significance of the doctors' connubial path to prosperity. Could it be that the main variable was the structure of the middle class?

It is also regrettable that Drees's sources break off at 1900, precisely when the German medical profession began a protracted and, by most accounts, successful battle for autonomy, prestige, and high incomes. The anti-physician animus in contemporary German society and the radical questioning of academic medicine by many social historians must be kept in mind when reading even the most scrupulous and well-informed histories of the German medical profession, such as this one or Claudia Huerkamp's pathbreaking work on doctors in Prussia. The triumphal Whig history of medicine taught and written in German medical faculties certainly deserves correction, but social history that reduces the achievements of modern medical science to flim-flammy (the root meaning of "prestige") is not likely to persuade a powerful professional establishment to rethink its role in humanistic and socially responsible terms.

Still, it is hard to fault a young scholar for paying lipservice in a dissertation to reigning orthodoxies among German social historians about the medical profession. It is a strength of Drees's book that the central argument does not often interfere with the statistical evidence, which is scrupulously presented and intelligently discussed. Historians of nineteenth-century Europe will find Drees's book a gold mine of interesting evidence, as well as an example of the frustrations of using regression analysis on disparate quantitative signifiers.

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ERNST NOLTE. *Nietzsche und der Nietzscheanismus*. Frankfurt a.M.: Propyläen. 1990. Pp. 303. DM 34.

Ernst Nolte has been the great deflector among his generation of German historians, enclosing Nazism in an "epoch of fascism," assimilating the Holocaust into the Gulag, and turning Nietzsche contra Wagner into Nietzsche contra Marx. Arguing that it was through Marxism that Nietzscheanism achieved its political character, this work expands on Nolte's 1960 article "Marx und Nietzsche im Sozialismus des jungen Mussolini," portraying Mussolini's "synthesis" of 1913 as the intellectual event that prepared the ground for the murderous confrontation between two opposing concepts of "annihilation" (pp. 265, 193).

Conceiving of Marx and Nietzsche as rival "prophets" (p. 9) whose agendas condemned vast numbers, the book serves as a preface to Nolte's recent *Der europäische Bürgerkrieg 1917–1945* (1987). As neither Marx nor Nietzsche took cognizance of the other, Nolte is forced to operate at a highly speculative level. He stresses their common Hellenism, their "intellectualism," (p. 4) and their opposing utopian visions of a classless society and the Superman, deeming such links sufficient to cast them as great unknown antagonists. In straining to find parallels, Marx appears on as many pages as Bismarck, Wagner, Burckhardt, Darwin, and Overbeck combined. Yet Nolte ignores the obvious: the generational revolt of the young Schopenhauers against their "fathers," the Young Hegelians. By overshadowing Nietzsche's critique of the Right with a conjectured antagonism to Marx, Nolte deflates Nietzsche's Wagnerian apostasy, ignores his Swiss exile, and downplays his harried identification with the doomed liberal Kaiser in 1888.

The motto of the book, "Nietzsche as battlefield," (p. 10) is meant to evoke a Nietzsche alive to all the issues of his day, a prophet more farsighted than Marx, but the sentence in Nietzsche's works from which the phrase is lifted refers to the break with Bayreuth in 1876, which Nolte ignores. Nietzsche's "battlefield" is emptied of Wagnerians, anti-Semites, and nationalists to dramatize a confrontation between Nietzsche and Marx. Nolte assesses the turn to

"praxis" (p. 77) that gripped Nietzsche in his last months of sanity in a political void. Seeking to level the playing field between Nietzsche and Marx, he takes literally Nietzsche's "party of life" as the founding of a "party," rather than as a clarion call to the emerging international avant-garde (p. 83).

A Heideggerian disdain for the supposedly "shallow" nineteenth century allows Nolte to treat it as a mere parenthesis between the two European civil wars, 1793–94 and 1917–45. At the end of the "world civil war" (p. 80), Nolte targets not the "new Nietzsche" of recent decades but the depoliticized, "gentle" Nietzsche of early Cold War existentialism. Whereas Walter Kaufmann's *Nietzsche: Philosopher, Psychologist, Antichrist* (1950) disassociated Nietzsche from the Nazis by repudiating Bauemler's view of Nietzsche the "politician," Nolte rehabilitates the "tough" Nietzsche of the 1930s (claiming that Bauemler was "not only" a Nazi, p. 17).

Something is certainly learned from viewing Nietzsche through a Marxist/anti-Marxist perspective, particularly in understanding the interaction of Nietzscheanism and Marxism at the turn of the century. Nolte's book, however, remains a prologue to its own argument, interested in the post–1914 politicized Nietzsche, yet conveniently concluding just when Left Nietzscheanism emerged as a force. Had Nolte filled in the missing years of his schema, 1914–17, he would have confronted the "civil war" mentality within German Nietzscheanism, as operative here as between Nietzscheanism and Marxism. In the end the book sheds more light on the confusions of the world of the young Heidegger than on the actual world of Nietzsche. The Duce's bleatings on Nietzsche may be less telling than Nietzsche's reaction to his chance encounter with Giuseppe Mazzini.

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MARIA MAKELA. *The Munich Secession: Art and Artists in Turn-of-the-Century Munich*. Princeton: Princeton University Press. 1990. Pp. xix, 205; 107 plates. \$35.00.

Maria Makela's book is a significant contribution to a field that has been largely ignored by Francocentric American art historians. Virtually a companion piece to Peter Paret's pathbreaking *Berlin Secession: Modernism and Its Enemies in Imperial Germany* (1980), the book presents a comprehensive view of the importance of art in the development of Munich as a cultural metropolis after the unification of 1871 undermined Bavarian political autonomy, causing both the state and the city to place a premium on maintaining Munich's artistic preeminence within the new nation. Together, the books provide instructive analyses of the ways in which differing political configurations in Bavaria and Prussia affected the emergence of art forms and institutions. Although Makela does not

match the breadth of Paret's historical analysis, particularly of the interaction between aesthetic values and right-wing political fears, she presents a convincing argument for the national liberal economic and political base that supported the great art exhibitions in the 1880s and 1890s. Art was perceived as an important commodity on the free world market that brought income to the city through sales abroad and elaborate exhibitions that attracted the wealth of tourists. The exhibitions also functioned, Makela argues, within a populist liberalism that sought to counteract the effects of industrialization through popular enlightenment, in this case by making art accessible to the people. With copious statistics and meticulous documentation from government archives and exhibition records, Makela traces the process by which aesthetic challenges inspired by French impressionism, elitist ideals calling for smaller exhibitions, and economic realities of artistic overproduction combined to produce the Secessionist movement that first took its German form in Munich in 1892.

Cultural and art historians alike will find a wealth of information in this book about institutional developments in Munich and a useful introduction to artists who are almost unknown in America. Written in a fluent narrative style, it is well illustrated with black-and-white plates. My praise for the book is tempered by the wish that Makela had examined her assumptions about the "progressive" nature of the avant-garde. This is a vexing problem because the nationalistic and anti-Semitic attacks on modern art, which she discusses, have traditionally validated its claims to be progressive, but Makela herself makes a strong case for the Secessionists—the modernists of their time—being elitist, antiurban, and authoritarian while the traditional exhibition groups were egalitarian, pluralistic, and committed to serving both a wider group of artists and public. Also troubling is a misogynistic aspect in the erotic works of the Munich Secessionists. Makela discusses this eroticism only in terms of the artists' frustrations within a repressive society, particularly in Catholic-dominated Bavaria. The number of negative depictions of women, however, suggests that she might also have considered the threat to artists posed by the gathering strength of the women's movement and the surprising increase in the number of women artists. Finally, it is oddly disturbing to find Makela so caught up in the mystique of the avant-garde canon—even while trying to wrench it from its French base—that she justifies the central section on Secessionist work by arguing that it was important for what it led to, namely Wassily Kandinsky and the *Blaue Reiter* group, and not for what it was.

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RICHARD J. EVANS. *Proletarians and Politics: Socialism, Protest, and the Working Class in Germany before the First World War*. New York: St. Martin's. 1990. Pp. xii, 196. \$49.95.

Concluding one of the six essays in this collection, Richard J. Evans encapsulates his guiding theme in a sentence: "Explorations of the origins and nature of workers' political commitment must in the end be rooted in an understanding of the structures and pressures of their everyday lives" (p. 69). It is a point that Evans pursues via two avenues. In his first five chapters he critically scrutinizes recent writings on the proletariat of pre-1914 Germany. Then, in the final and lengthiest piece, he presents an example of what his own approach to the subject may accomplish.

The initial essays, mostly revisions and consolidations of earlier reviews, offer Evans's negative lessons on working-class history. It should not be "teleological" (p. 28). Especially in his discussions of "the crowd in German history" and "the making of the German working class," the author underscores the drawbacks of imposing a straitjacket of historical stages on events that cannot be exclusively confined to "transitional" or "mature" phases (pp. 31–32). Neither should interpretations be ideological, in the sense of operating within the reform/revolution dichotomy. In the chapter on "socialist women and political radicalism," Evans shows that conflicts within the German labor movement over women's issues demand a more complex analysis than is afforded by the traditional model of a left/right division symbolized by the opposition between Clara Zetkin and Lily Braun. He also argues the case for distinguishing between socialist history and the history of laboring people throughout the book.

Evans's first three chapter titles, alluding to the landmark works of Louis Chevalier, George Rudé, and E. P. Thompson, turn out to be a distraction. He only passingly invokes their theses and does not rigorously examine how they work under German conditions. It is the author's own judgments that give weight to these thematic reviews. More valuable, however, is his attempt in the final essay, "Proletarian Mentalities: Pub Conversations in Hamburg," to fashion the kind of working-class history he says is needed.

Properly allowing for the prejudices of his sources, the reports of plainclothes police observers, Evans reveals a blue-collar population more separated from socialist doctrine than most portraits have suggested. Hamburg workers liked to gamble. In certain situations they approved of violence and theft. In politics they prized unity above all else and saw it validated by the party congresses that the literature commonly dismisses as empty ceremonies. Class identification was fundamental to them, and they suspected revisionism because it was the creation of intellectuals, not for its programmatic content. The negative circumstances of work and living sustained their resent-

ment against the existing order, even if secular economic trends gave contrary indications. It is necessary to recognize the conditionality of working-class attitudes.

Evans's study of Hamburg rightly warns against affixing categories of socialist ideology to workers' outlooks that were responsive to day-to-day contingencies. It leaves questions, however, concerning the continuity of political loyalties. Social Democratic workers did, after all, stay true to their banner. In the end, the analysis better explains enthusiasms than long-term commitments. If "[m]uch depended on the situation of the moment" (p. 186), it remains to be explained why some moments had more lasting effects than others.

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EBERHARD DEMM. *Ein Liberaler in Kaiserreich und Republik: Der politische Weg Alfred Webers bis 1920*. (Schriften des Bundesarchivs, number 38.) Boppard am Rhein: Harald Boldt. 1990. Pp. viii, 476. DM 74.

Scholars have written a great deal about the failure of German liberalism in the early twentieth century. Eberhard Demm's political biography of Alfred Weber (1868–1958) adds an interesting new dimension to this familiar story. Prepared originally as a *Habilitationsschrift* at the University of Paris (Nanterre), this carefully written study of Max Weber's younger brother analyzes the political development of an influential academic liberal in difficult times.

Alfred Weber is worth more than a footnote, Demm argues, because his political ideas mirror so many trends in mainstream German liberalism between 1900 and 1920. Born into a prominent liberal family, Weber grew up in a prosperous upper-middle-class Berlin environment, enjoying all the personal and educational advantages his social status bestowed. He studied law and economics in preparation for the judiciary, but eventually turned to an academic career in economics, teaching first at the German University in Prague (1904–07) and then at Heidelberg University (1908–14, 1919–33). Weber earned scholarly recognition as an economist and cultural sociologist; his engaging classroom teaching style also won him widespread student acclaim.

Yet Weber never fit conventional liberal stereotypes. His restless, critical spirit led him to condemn conventional Christianity, middle-class morality, and Bismarck's repressive political system. His nervous impatience often provoked clashes with colleagues and political friends, and even with his own brother. Although this mercurial academic often sounded like a democratic progressive when talking about social reform or individual rights, he sometimes came across as a reactionary nationalist when embracing social Darwinism, racial imperialism, a German-dom-

inated Central Europe, and other *völkisch* ideas. Demm's lucid description of the sharp incongruities between Weber's liberal-democratic rhetoric and his elitist, reactionary beliefs is one of the major strengths of this book.

Another is its explanation of Weber's abject failure as a party politician. The most revealing episode occurred late in 1918, when Weber helped to found and then provisionally chaired the new German Democratic Party, a Left-liberal coalition of prewar Progressives and National Liberals. In this critical role his inexperience, murky ideas, and cranky inability to compromise led him to reject the inclusion of conservative liberals in the new party, thus perpetuating the enervating liberal divisions of the prewar era. Weber's political miscues cost him his leadership roles in the Democratic Party and led him to withdraw from active party politics in early 1919. He returned that year to Heidelberg University a disillusioned and embittered man.

Demm's conventional biography is based on a wide array of primary sources and secondary works, most notably the extensive correspondence between Weber and his longtime companion Else Jaffe-Richthofen. An excellent collection of photographs, useable index, and thorough chronology also enhance the book's value. But Demm's work is compromised by several self-imposed limitations. The author's decision to conclude his analysis in 1920, for example, leaves many unanswered questions about Weber's political path through the Weimar Republic, the Third Reich, and post-1945 Germany. By concentrating on Weber's political ideas, moreover, Demm frequently loses sight of the broader political context. Nevertheless, this wide-ranging monograph provides useful confirmation for those scholars who have located the tragedy of German liberalism in flawed ideas and individual failure.

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KAREN HAGEMANN. *Frauenalltag und Männerpolitik: Alltagsleben und gesellschaftliches Handeln von Arbeiterfrauen in der Weimarer Republik*. Bonn: J. H. W. Dietz. 1990. Pp. 877. DM 98.

Weimar Germany's experiments in democratic politics, economic rationalization, the expansion of educational opportunities, increased provisions by the welfare state, and initiatives for sexual reform did little to improve the status of working-class women. This is the conclusion that Karen Hagemann draws from this exhaustively researched study of the daily lives, work, organization, and politics of women in Weimar's Social Democratic "milieu." Moving between accounts of national developments and a detailed look at Hamburg, this remarkable study is not only a major contribution to Weimar working-class history but it also forces us to disaggregate "modern-

ization" and "rationalization" along the axis of gender. By leaving these categories between quotation marks throughout the text, Hagemann makes clear her deep misgivings about their adequacy for describing the experience of a working class that includes not only men but women.

Hagemann provides overwhelming evidence that the image of the "new woman," the sexually emancipated flapper with her bobbed hair and cigarette, had little to do with the lived experience of most working-class women. Although some younger women had access to greater employment opportunities, particularly in white collar jobs, they remained locked in a labor market rigidly segmented by sex. Their chances for upward mobility or economic independence were extremely limited.

In the home, at the workplace, in trade unions, and in the Social Democratic Party (SPD), women confronted a "modernized proletarian anti-feminism." The legal guarantee of the franchise and equal political rights did not eliminate the tension between the rhetorical commitment of male comrades to women's equality and their practical expectation that hierarchical, patriarchal structures would survive unaltered. In Hagemann's account, matters were made no better by the SPD's move away from the prewar theoretical position that described women's entry into political consciousness via wage labor. Rather, a "gender-specific" politics that championed an equality in difference underscored normative conceptions of women as housewives and mothers, greatly restricting the emancipatory visions available to women.

Much of Hagemann's account of "women's daily life" will provide few surprises to those who have studied the history of working-class women in other advanced industrial countries in the late nineteenth and early twentieth centuries. With some variations, her description of women's daily life and survival strategies in Weimar offers a picture outlined by historians of working-class women in other parts of Europe and the United States. It has never been drawn, however, in such rich detail for the German case. Hagemann consults an enormous range of archival and published sources, supplementing them with fifty oral histories of working-class women in Hamburg. These interviews set in sharp relief the gap between socialist theoretical commitments to women's emancipation and the realities of working-class women's lives. In addition, when Hagemann moves beyond daily life to discuss sexual reform, the politics of family planning, population size, theories of early childhood socialization, and the array of responses to the "woman question" offered by Social Democrats, she provides an account of key elements in Weimar social and political history which have never before received such thorough treatment. Indeed, she illuminates the many points at which "men's politics" (*Männerpolitik*) were structured around concerns stemming from "women's daily life" (*Frauenalltag*).

Hagemann concludes by speculating that the expe-

rience of Weimar as a series of uninterrupted crises was far more pronounced for women than for men. This in turn may have contributed to the "sociopsychological" willingness of women to accept National Socialism. Here she echoes the arguments of Renate Bridenthal, Claudia Koonz, Atina Grossmann, and others that Weimar women who moved toward the Nazis sought an "emancipation from emancipation," an alternative to Weimar's unfulfilled promises, an uncertain future in exchange for an intolerable present.

A brief review cannot do justice to the magnitude of Hagemann's achievement. Although parts of her story have been told elsewhere, there is no other so comprehensive, thoroughly documented, and compellingly told account of a gendered Weimar working class. For those interested in the history of working-class women in these years, Hagemann's work will not be the last word, but for anyone seeking to pursue this subject further, it will be a mandatory starting point.

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BERNARD P. BELLON. *Mercedes in Peace and War: German Automobile Workers, 1903–1945*. New York: Columbia University Press. 1990. Pp. xv, 356. \$37.50.

The German automobile industry had its origins in the 1880s in the workshops of Gottlieb Daimler and Karl Benz. In 1903, the Daimler motor company built a new factory at Untertürkheim, near Stuttgart. Bernard P. Bellon takes the establishment of that plant as his point of departure, chronicling labor relations at the Daimler enterprise from 1903 through its merger with Benz in 1925–26 to the collapse of the Third Reich. The story Bellon tells is not one intended to gladden the hearts of the public relations personnel of Daimler-Benz.

Bellon's coverage of the period from 1903 to 1945 is uneven. Almost half of his study is dedicated to the six years from 1914 to 1920. In 1914, the Daimler motor company employed not quite four thousand men, the majority skilled and unionized, in the painstaking production of small numbers of luxury and racing cars. The coming of war brought major changes as the company became the biggest producer of airplane engines for the German military. Daimler's work force swelled to over twenty thousand, almost three thousand of whom were female. Bellon stresses the huge profits the company made during the war through a combination of dishonest pricing and the exploitation of its employees, especially poorly paid women.

When the war ended, Daimler was left with massive overcapacity and a radicalized work force. Communists and independent socialists replaced moderate social democrats as the foremost representatives of

the Daimler workers. The turbulent labor relations of the immediate postwar period culminated in a lockout in summer 1920. Bellon provides a detailed narrative of that confrontation. The lockout proved to be a crushing blow for labor. Taking advantage of state intervention intended to quell worker opposition to a new tax on wages, the company was able to use the lockout both to slash the size of its work force and to purge it of radicals.

Beyond 1920, Bellon's account becomes sketchier, especially for the remaining years of the Weimar Republic. Bellon gives a brief description of the rationalization that preceded the Great Depression, then devotes the rest of his book to the interaction of Daimler-Benz and National Socialism. In his contribution to the debate on the relationship between big business and the Nazis, Bellon is a harsh critic of the capitalists. He puts the worst possible construction on the evidence. Viewing the leaders of Daimler-Benz as driven by a "lust for profit and expansion" (p. 265), Bellon sees them as willing accomplices and beneficiaries of the Third Reich. Recounting their wartime acquisitions and their exploitation of foreign workers and concentration camp labor, Bellon casts the company's directors in the role of war criminals. He denies that much *Gleichschaltung* took place at Daimler-Benz. He argues that the concern remained largely able to manage its own affairs, even to the extent of acquiring in 1942 as its new leader a man whose wife was half Jewish. Bellon ascribes the lack of resistance to most Nazi dictates to an essential harmony of interests between the concern and the regime.

Bellon makes extensive use of records from the Daimler-Benz archive. He had to overcome, however, the firm's initial refusal to grant access to documents relating to the sensitive years from 1933 to 1945. Even with this access, the last half of Bellon's book remains less well documented than the first. Particularly on relations between Daimler-Benz and the National Socialists, the evidence Bellon presents is sometimes scanty. Still, he has produced a useful case study, offering accounts of a number of intriguing encounters of capital, labor, and the German state.

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INGEBORG FLEISCHHAUER. *Der Pakt: Hitler, Stalin und die Initiative der deutschen Diplomatie 1938–1939*. Berlin: Ullstein. 1990. Pp. 552. DM 58.

The fiftieth anniversary of the Nazi-Soviet Pact of 1939 drew great public attention, in part because of developments in the Baltic states, one of the objects of that pact, and in part because the Soviet Union officially recognized the secret protocol attached to it, a protocol previously denounced by Moscow as a fabrication. Ingeborg Fleischhauer presents in this detailed volume a new account of the origins and

significance of the pact based on some of the published literature, recently available Soviet documents, research in German archives, and, most important, the papers of the German ambassador to the Soviet Union at the time, Count Friedrich Werner von der Schulenburg, which she discovered.

The author's interpretation may be summarized as follows: she dismisses as unsubstantiated all references to approaches by the Soviet Union to Germany in the period from 1933 to 1939, insisting instead that it was Stalin who broke off all contacts with Germany and staked Soviet safety on alignment with the West. The origins of the Nazi-Soviet Pact are attributed to the initiative of German diplomats who are supposed to have persistently pushed for this with both their own government and the Soviet government in the face of enormous reluctance at the top in both Berlin and Moscow in the hope of thereby providing not only for the security of the two countries but also a stable order in Eastern Europe. She argues that the Soviet Union was dubious about all the approaches that, she insists, came from German diplomats and Berlin, and that Moscow seriously wanted an agreement with the Western powers. Similarly, the author states that, until the last moment, Stalin wanted a military agreement with the West and preferred a peaceful settlement, which he hoped for even after the signing of the pact.

Whatever the interest of the new material from Schulenburg's papers adduced by the author, the theses she advances are hardly convincing. The prior approaches by the Soviet Union to Germany cannot be discarded so easily. The extensive material on Soviet aggressive designs on all or parts of the countries of Eastern Europe also cannot simply be ignored. Because Hitler, not Stalin, ordered the end of German experimentation with chemical warfare inside the Soviet Union, one cannot dismiss Soviet interest in continued cooperation with the Germans so readily. The author ignores, on the one hand, the published evidence on the Soviet refusal to take advantage of Romania's offer of transit rights for Soviet forces to aid Czechoslovakia in 1938; on the other hand, she cites but totally misreads the secret protocols of the Anti-Comintern Pact of 1936. Repeatedly, she mentions that pact's alleged offensive alliance character, yet the texts show that it was nothing of the sort, as the Soviet government knew at the time.

Unfortunately, especially in view of the care the author has taken to unravel some of the technical problems of the documents, Fleischhauer neither includes a map nor knows the geography of the area she deals with. She writes often about the border between the Carpatho-Ukraine (Ruthenia) and the Soviet Union and the importance of developments on that border, but that demarcation exists only in her imagination. Similarly, her discussions of the so-called Vilna corridor make clear that she confuses the

Vilna area of prewar Poland with the Mariampole area of Lithuania.

It is possible that the German diplomats whom she cites were short-sighted enough to believe that a Nazi-Soviet agreement would stabilize the situation in Europe, but they were surely alone in that view. This brings up a basic flaw in this thoughtfully argued defense of Stalin's policy in 1939. Fleischhauer refers to the soundings between London and Berlin in 1939 but avoids any reference to their contents. The British were insisting that, as a prerequisite to any new agreement, Germany first had to take a step back from its breach of the Munich agreement, that is, restoring the independence of Czechoslovakia, and Germany also had to refrain from any aggression against Poland. Hitler had no trouble understanding the point and rejected such ideas. Stalin also understood it. As documents from the Public Records Office indicate, he explained to the British ambassador in July 1940 that "the U.S.S.R had wanted to change the old equilibrium . . . but that England and France had wanted to preserve it. Germany had also wanted to make a change in the equilibrium, and this common desire to get rid of the old equilibrium had created the basis for the rapprochement with Germany."

Recognition of the disappearance of Czechoslovakia and sharing in the disappearance of a number of other European countries, and then urging the Western powers to accept the new status quo, is a very different policy from that which Fleischhauer attributes to the Soviet Union. A brief look at the military contacts between the Soviet Union and the Western powers during the years they were allies fighting for their lives in World War II might have enlightened her account of Soviet-Western military talks in 1939. One should not dismiss the respected Soviet scholar Vyacheslav Dashishchev as the "*enfant terrible*" of Soviet historians (p. 441, n. 32). His research in Soviet archives has convinced him that the Congress of the Communist Party in March 1939 was designed to prepare the Soviet Union for an agreement with Germany on which Stalin had by then already decided. Dashishchev closed his report on the subject at a conference in Berlin on the fiftieth anniversary of the beginning of World War II with the comment that this shed a very different light on the negotiations between the Soviet Union and the Western powers in the summer of 1939.

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HORST BOOG *et al.* *Das Deutsche Reich und der Zweite Weltkrieg*. Volume 6, *Der globale Krieg: Die Ausweitung zum Weltkrieg und der Wechsel der Initiative 1941-1943*. Stuttgart: Deutsche Verlags-Anstalt. 1990. Pp. xix, 1184. DM 78.

This is the pivotal volume in the German Military History Research Office's projected ten-volume series, *The German Reich and the Second World War*. Herein, Japan and Germany unleash the world war by declaring war on the United States and, through their defeats at El Alamein, Guadalcanal, and Stalingrad, lose the initiative on all fronts. Germany, however, as the "and" in the series title indicates, is presumed not to have been a viable contender in the world war, even at the outset. The volume, therefore, treats the world war as a storm building in the background while for Germany the clock ran out on the European war Hitler provoked in 1939.

The first two parts, a quarter of the text, deal with the world war. Part 1, drawing on the standard British and American literature, provides the background of the anti-Axis coalition and summarizes Allied planning from the Atlantic Conference to Casablanca. With regard to Hitler's approach to the war, two phases are postulated: a "time of hope" (to August 1942), during which he believed his original strategy—forcing the British to concede defeat by a victory over the Soviet Union—would enable him to conduct a global war, and a "time of obstinacy" (to the year's end), in which he refused to admit he had been mistaken. Part 2 traces Japan's course into the war and to the turnabout at Guadalcanal.

Part 3 resumes the war at sea in the Atlantic begun in volume 2 and carries it through the periods of the U-boats' greatest successes: the January to July 1942 campaign in the unprotected U.S. coastal waters and the late 1942 to early 1943 wolf-pack attacks on the Atlantic convoys. Unlike some British and American writers, the author of this part, Werner Rahn, attributes the decline in sinkings that began in early 1943 less to ULTRA than to the air cover the Allies provided for the convoys.

In part 4, Horst Boog sets the scene for the Allied strategic bombing offensive yet to come. Starting with an analysis of British and American doctrine in the 1930s, he describes the operations, mostly British, from 1940 through 1942. In the same time frame, he points out that the German air force, at first by preference, later in the expectation that the war would be over before strategic bombing could become effective, concentrated on supporting ground operations, neglected fighter production, and left air defense largely to the antiaircraft artillery.

Reinhard Stumpf, in part 5, picks up the war in the Mediterranean and North Africa (begun in volume 3) at the start of Erwin Rommel's countermarch through Cyrenaica in January 1942 and takes it to February 1943 when, after the third battle of El Alamein and the British and American landings in Algeria, Rommel's recently created Army Group Africa was confined to a "bridgehead" in Tunisia.

The series' overarching problem, a German past that refuses to become just history, is most evident in the handling of the German-Soviet war. The authors of volume 4 devoted over a thousand pages to the

campaign of 1941 and concluded that a victory of any kind over the Soviet Union was no more than a chimera and that the blitzkrieg proved itself hopelessly inadequate in the first joined battle, at Smolensk in July 1941. In short, they argued that Germany never had been—or could be—capable of defeating the Soviet Union. In part 6 of the present volume, Bernd Wegener, after analyzing the campaign of 1942, reaches a substantially different assessment, namely, that a collapse of Soviet resistance was twice “within the realm of possibility” (p. 1102): in the fall of 1941 and again in the summer of 1942. Therefore, in his estimation, the “point of no return” reached at Stalingrad could not have been the preordained terminus of a road entered in June 1941. Should that be so, then Germany must also have been something more than a mere bystander in the world war, at least until after August 1942. Because the Soviet condition during the 1942 campaign will not be considered until volume 8, having been put off pending new disclosures from glasnost, readers will have to wait to learn whether the issue can be resolved. For the present, it appears that while glasnost, to which the Soviet military has thus far shown no strong commitment, may not positively confirm the “realm of possibility,” the known facts and certain pre-glasnost evidence already support Wegener.

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JAY W. BAIRD. *To Die for Germany: Heroes in the Nazi Pantheon*. Bloomington: Indiana University Press. 1990. Pp. xvii, 329. \$35.00.

The myth of heroic death occupied a central place in National Socialist liturgy because it beckoned Germany's redemption in a friendless twentieth-century world. In fascinating detail, Jay W. Baird shows how the Nazis redescribed the field of battle to create a stock of Christ-like figures who validated the “resurgent life force of the Fatherland” (p. 2). Familiar Nazi heroes such as Albert Leo Schlageter and Horst Wessel fit into a genre that began with the memorialization of fallen youth at Langemarck in 1914. Notions of sacrifice, resurrection, and return allowed the Nazis to locate the hardships of political struggle in a larger chain of life. Baird's compelling analysis of SA street fighters demonstrates how Joseph Goebbels transformed questionable figures into heroes for the Third Reich. Rather than whitewash the dead, Goebbels used their delinquency to depict the moral agony of the Weimar Republic and to suggest so many more stations on the via dolorosa of National Socialism.

Baird blends first-rate historical reconstruction with expert cultural analysis. He explores novels, songs, and films as well as the public ceremonies and private letters that have left traces in the archives to provide a comprehensive picture of Nazi commemo-

ration. Although the hardships of war eventually diminished the appeal of excessively choreographed ceremonies, Germans continued to reach for heroic writers such as Walter Flex and to respond to partisan heroes such as Wessel up to 1945. Baird then turns his attention from the heroes to their troubadours, the poets Gerhard Schumann and Hans Baumann and the film director Karl Ritter. It is one of the strengths of this highly informative book that it carefully examines both the message and the way the message was broadcast. A final chapter on the myth of death in World War II fails to re-create the ruthless engagement on the eastern front, but it brilliantly probes the efforts to restabilize the National Socialist meaning of death in the face of the private reformulations of grieving soldiers and relatives.

This study adds up to an original examination of culture and meaning in Nazi Germany. Its results, however, fall somewhat short of the subject matter. Baird accepts uncritically all sorts of conventions about heroes and Nazis. A guiding assumption of the book seems to be that the Nazis misused heroes, either by blurring heroism and barbarism (p. xi) or by unleashing “the demonic in man” (p. 244). This was so, Baird suggests, because the trauma of defeat and revolution in 1918 called for a more radical cult of heroism. But the distinction between Nazi and other heroes, satisfying as it may be, is maintained by muddling other differences. It is not clear why all heroes, who are claimed by public virtue and will not be released to private tragedy, do not in some way brush the barbaric. Repeated emphasis on the desolation of the Weimar period is also troubling. It enhances Baird's subjects with an honor that they do not deserve and unwittingly encourages the reader to consider Nazism simply as a function of Germany's humiliation. The result is a misreading of German nationalism, which was more confident and grandiose than Baird allows and which threatened rather than was threatened by annihilation. “To die for Germany” always meant two things. Unfortunately, Baird does not link the characteristics of Nazi heroes with the fate of Nazi victims. He thus makes death the subject of his fine study but fails to place it at the heart of National Socialism.

PETER FRITZSCHE
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JOSEF MÜLLER. *Die Gesamtdeutsche Volkspartei: Entstehung und Politik unter dem Primat nationaler Wiedervereinigung 1950–1957*. (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 92.) Düsseldorf: Droste. 1990. Pp. 445. DM 78.

This book is a political history of the neutralist *Gesamtdeutsche Volkspartei* (All-German People's Party,

or GVP). Josef Müller traces the GVP from its origins as the *Notgemeinschaft für den Frieden Europas*, an extraparlimentary force against German rearmament, to its eventual absorption by the Social Democratic Party in 1957.

The GVP survived a mere five years. It fared badly at the hands of voters in the federal elections of 1953, receiving less than 1.2 percent of the total vote, well below the 5 percent hurdle for attaining representation in the Bundestag. Splinter parties with tiny constituencies come and go with such rapidity in Germany that they are rarely worthy of full-length scholarly treatment. The GVP, however, is exceptional, for its influence extended well beyond the fringes of power: it was a highly visible and strident opposition force working to undermine Chancellor Konrad Adenauer's Western integration policies during the public debates over the European Defense Community Treaty and NATO membership; and its co-founder, Gustav Heinemann, became Federal president (1969–74) during the *Ostpolitik* era.

Because of Heinemann's national prominence, previous scholarship on the GVP has tended to be largely biographical. Müller corrects this bias somewhat by focusing equally on Helen Wessel, the Center Party leader and co-founder of the GVP. Wessel "stood within the Prussian tradition of balance-of-power politics" (p. 82), and her position was close to that of Jakob Kaiser (Adenauer's opponent in the Christian Democratic Party), who saw Germany as a "bridge" between East and West. Müller also gives pithy synopses of other GVP leaders, and offers an excellent glimpse into the party's executive board, where interesting and occasionally heated political discussions took place.

Missing from Müller's analysis, however, is an adequate discussion of the GVP's constituency. Who voted for the GVP? At a time when the "ohne mich" (count me out) fever was running high, why did so few Germans back a neutralist party? With several splinter parties competing for the neutralist vote, why did so few rally to the GVP? None of these questions are squarely faced, a major failing in a book that purports to trace the rise and fall of a political party. Nonetheless, the reader senses that the West German government was able to debunk the GVP, as it did all opposition parties, by tarring it with the brush of political extremism. The GVP painted itself into a corner by flirting with the left-leaning *Bund der Deutschen* during the election campaign of 1953. Adenauer did the rest. "Have you heard the Gustav-Wessel song?" he asked during one of his famous "tea talks" with the press, alluding to the notorious Horst Wessel song of the Nazi Party (p. 321).

Müller tends to see international events through the prism of the GVP's party platform; this might help explain why he accords so little attention to the party's fate. Still, this book offers an excellent glimpse

into the inner workings of the GVP, and it clearly supersedes its predecessors.

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BEATRIX W. BOUVIER. *Zwischen Godesberg und Grosser Koalition: Der Weg der SPD in die Regierungsverantwortung; Aussen-, sicherheits- und deutschlandpolitische Umorientierung und gesellschaftliche Öffnung der SPD 1960–1966*. (Veröffentlichungen des Instituts für Sozialgeschichte Braunschweig.) Bonn: Dietz. 1990. Pp. 352. DM 78.

The unification of Germany in October 1990 led to all-German elections two months later. For the Social Democratic Party (SPD), in opposition since 1982, the results were disappointing. Their initial hopes of establishing a strong electoral base in the former German Democratic Republic were shattered when close to a majority of East Germans voted for the Christian Democratic Union (CDU) in a series of electoral contests culminating in the historic election of October.

There are historical parallels in the postwar evolution of the SPD. In this book, published prior to unification, Beatrix W. Bouvier deals with one such period, 1960–66. The SPD, not having gained the chancellorship since 1949, decided that its fundamental and adversarial opposition to the pro-West foreign, defense, and inter-German policies of the CDU, its Bavarian affiliate, the Christian Social Union (CSU), and the CDU/CSU coalition partner, the Free Democratic Party (FDP), had to be scrapped. Instead, the SPD embarked on a policy of consensual politics or, as Bouvier puts it, "togetherness" (*Gemeinsamkeit*). In effect, the SPD swung into a pro-West position in which its differences with the CDU/CSU–FDP narrowed considerably. This position, based on the reformist Godesberg basic program of 1959, ended successfully in 1966 when the SPD joined a CDU/CSU–led grand coalition cabinet and the small FDP was the lone opposition party.

Although the period from 1960 to 1966 has been studied by other specialists, Bouvier analyzes familiar events from the perspective of whether opposition parties should play an adversarial or constructive role in a pluralist democratic system. She opts for the latter approach in the German setting of the 1960s, although she acknowledges that voters may find too much consensual politics boring.

Her thesis, substantiated by meticulous research including access to the protocols of SPD presidium and executive committee meetings (normally not available to scholars), indicates that the SPD made itself more respectable to the voters through its role of constructive opposition. In a country whose political culture does not tolerate easily parties or groups that stand in sharp opposition to the status quo, the

SPD foreign policy goals in the 1960s eventually paid off once voters saw that the party was capable of governing the country.

After a brief historical survey of SPD foreign and defense policies from 1949 to 1959, Bouvier devotes six chapters to the rapprochement of the SPD's policies, ranging from German-French relations to national emergency legislation, with those of the governing parties. In a period of temporary de-ideologization and apolitical attitudes on the part of many citizens, the SPD strategy of seeking respectability through its advocacy of a grand coalition with the CDU/CSU found support within the SPD, but also opposition, especially among left-wing rank-and-file members. Bouvier assesses the degree of intraparty support and opposition as well as reactions from other party leaders and the media as she studies various SPD conventions, regional and national elections, and parliamentary debates in the Bundestag. In this survey, which hardly touches on domestic policies, SPD leaders Erich Ollenhauer, Willy Brandt, Herbert Wehner, and Fritz Erler play an important role, as do CDU chancellors Konrad Adenauer and Ludwig Erhard.

Bouvier has made an important contribution to the study of the SPD and the role of opposition parties in a democratic system. She is objective in her overall appraisal of political actors and events. Although times have changed, SPD leaders eager to recapture the national government in 1994 might profit from a careful reading of her book.

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ROBERT F. GOECKEL. *The Lutheran Church and the East German State: Political Conflict and Change under Ulbricht and Honecker*. Ithaca: Cornell University Press. 1990. Pp. xiv, 327. \$39.95.

The nonviolent revolution in the German Democratic Republic (GDR), which brought down the ossified Marxist-Leninist regime and paved the way for "re-unification" essentially on the terms set by the West German Federal Republic, stirred the imaginations of people everywhere. As one who was on the scene during the tumultuous fall and winter of 1989-90, I saw church people as central figures in the drama and recognized again the unique role that the Evangelical (Protestant) church had played during the GDR's forty-year existence. This seems most remarkable given the Lutheran tradition of political passivity, but political scientist Robert F. Goeckel shows clearly how over the years ideological antagonism between church and state evolved into a modicum of cooperation.

The book opens with a discussion of the regime's decision to allow the reconstruction of the Wilhelmine-era Protestant cathedral in Berlin (a ruin situated

in the government quarter) as symbolic of the rapprochement between church and state in the 1970s. Perhaps just as symbolic of the new order after the *Wende* (the "turn," the popular name for the revolution) is how the GDR government buildings now sit empty and the state's own temple, the luxurious Palace of the Republic, is in danger of being razed because of its asbestos content while the cathedral's glistening gold cross dominates the skyline. Then, in well-written, fast-moving chapters Goeckel accurately surveys the trends in East German church-state relations from the postwar Soviet occupation to the collapse of the communist state established there.

He shows that the relationship was relatively benevolent in the early years, but pressure on the church was gradually increased as the GDR pursued a policy of *Abgrenzung* (delimitation) regarding its western counterpart. With the formation of the Church Federation in 1969, the churches in East and West Germany were formally separated, but the east church still maintained some ties with the west church and was allowed to train clergy, repair and build churches, broadcast religious services, disseminate publications, and operate a network of social-service agencies. Nevertheless, during the ensuing two decades the relationship was an uneasy one and church leaders frequently dissented from and even criticized state policies.

Goeckel provides a lucid explanation of the actions by both parties, especially when it came to the state's two-sided strategy. Although seeking through compromise on some institutional issues to temper criticism by the church elite, the regime fostered disaffection among the grassroots by engaging in ideological education, militarization of the society, and discrimination against Christians. Also valuable are Goeckel's treatments of prominent church figures such as Albrecht Schönherr, Werner Krusche, Moritz Mitzenheim, and Heino Falcke. The one body that comes off looking very badly is the east Christian Democratic Union (CDU), which functioned largely as a passive instrument of transmission for the governing SED (Communist Party) and then was gradually shunted aside as the state's direct relations with the church leaders deepened. But in 1989 the CDU quickly changed course and participated in German unification. It is unfortunate that presentist language is used in the opening and closing chapters, as this unnecessarily dates the book, but still it is the best work available on the political tensions between church and state in the GDR. Placed in this perspective, the revolution of 1989 makes much more sense.

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MICHAEL PHAYER. *Protestant and Catholic Women in Nazi Germany*. Detroit: Wayne State University Press. 1990. Pp. 286. \$34.95.

Humanity's frail hold on morality probably will never cease to disturb us, and what better source of uncertain behavior is there to study than National Socialism? What better test of principle than the behavior of Germans who defined themselves primarily in religious terms? Who better than women thus committed, the historically male-defined "better half" of the human race? Michael Phayer attempts such an evaluation. He asks to what extent members of the major Protestant and Catholic women's organizations opposed Nazism and weighs the importance of their religious ideals, as distinct from other possible motivations, to their political posture.

He finds that Protestant women were initially more receptive to nationalism, which they embraced "with almost millennial expectations" (p. 77). The first years of Nazism were, for them, "A Dream Fulfilled" (the title of chapter 4). By contrast, organized Catholic women stayed aloof at first, although they were less alarmed about Nazism than about communism. The Vatican's concordat, signed six months after Hitler came to power, gave these women hope, if not enthusiasm, to participate fully in the new social order.

The Nazis sought to bring most associational life into their own party organizations, seducing or bullying existing groups into cooperating. The Protestant and Catholic mother-care groups each negotiated with their Nazi counterpart in the hope of at least sharing the maternity supports they had pioneered. Each lost, and later objected to different aspects of racism that were increasingly being institutionalized. Protestant women, for example, supported sterilization laws as in the best interests of the *Volk*, while Catholic women clung to their religious teachings opposing intervention into reproduction. But neither group publicly opposed anti-Semitism.

Protestant and Catholic women's associations took an active part on behalf of their churches regarding control over religious ideology. In the Protestant church struggle that pitted the nazified German Christians against the resistant Confessing church, the Protestant Ladies Auxiliary (PLA) joined in with the latter. The fight of its leader, Agnes von Grone, to keep the PLA autonomous led to her expulsion from the party. The Nazis' attack on the Old Testament led Protestant women to hold Bible studies through workshops, which were periodically harassed by the government. Similarly, organized Catholic women rallied against Nazi attacks on their schools by assuming some of their functions, including teaching the catechism. The quietism sponsored by the concordat hampered any major organized struggle.

Phayer concludes that Protestant and Catholic women were essential to the churches' efforts to withstand dechristianization, but he claims that National Socialism succeeded in eroding those efforts and also in forestalling women's evolution toward wider social and economic roles. One exception is that Protestant women gained some access to church leadership, including ordination.

The major problem with this book is Phayer's insistence that, in their relationships with National Socialist parallel organizations, organized churchwomen were motivated by their faith rather than feminism, social class, or personal opportunism. He arrives at this conclusion by taking their proclamations at face value. Sometimes he undermines his own argument. For example, he contends that economic interpretations fail to explain the PLA because "once Germany's economic recovery was accomplished, most of the women identified with the auxiliary did not hesitate to loosen ties to national socialism over a noneconomic question" (p. 48). In this statement he first uses an economic interpretation and then rejects only its most reductionist form. Phayer also is unconvincing on the matter of individual motivation, as in the case of Agnes von Grone, whom he defends as a principled opponent of neopaganism. Claudia Koonz argued that von Grone's private war for power together with her otherwise staunch defense of Hitler translated instead into opportunism (*Mothers in the Fatherland* [1987]). There is room for disagreement here, but the bottom line is that one cannot be certain of motivation.

A word on language. Koonz's use of the term "Jewish ancestors" of Christian converts is preferable to Phayer's terminology, alternating "Christian Jews" with "Jewish Christians." If Jews are such, no matter what religion they choose, they are being defined as a race.

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LEE PALMER WANDEL. *Always among Us: Images of the Poor in Zwingli's Zurich*. New York: Cambridge University Press. 1990. Pp. vii, 199. \$29.95.

In this short book Lee Palmer Wandel presents an uncommonly clear, crisp, and elegant analysis of views toward the poor developed at the beginning of the Protestant Reformation in Zurich. She begins with two background chapters. The first is a swift but expert survey of perceptions of the poor displayed in the Bible, through the Middle Ages (particularly among the followers of St. Francis of Assisi), and among civic leaders influenced by Renaissance humanism. The second chapter is a fine analysis of Zurich, examining the geographic, economic, social, political, and religious components of the community at the time it adopted Protestantism. She then turns to her basic contribution: an analysis of three "languages" in which changing views toward the poor were expressed in Huldrych Zwingli's Zurich. A first "language" is theological, primarily as exemplified in three treatises by Zwingli published between 1523 and 1525. A second "language" is artistic, most often displayed in the pictures printed with those three treatises. A third "language" is legal, normally ex-

pressed in the texts of laws adopted by the government of Zurich to control poor relief in 1520 and in 1525. Wandel concludes that Zurich in these years witnessed a significant shift in religious views of the poor, from one symbolized by a "sacramental Christ" represented primarily by mendicant friars and individual givers of charity to one symbolized by a "social Christ" embodied in a city government that now closely defined those types of poverty deserving relief and monopolized delivery of that relief. Catholic opponents to this regime were attacked with vigor for having diverted resources from relief of the poor to art and high living. Anabaptist opponents to this regime were attacked with equal vigor for abandoning all social responsibilities to the poor by proposing to refuse payment of tithes. The book ends with complete texts of the two key statutes and an index.

Much of the strength of Wandel's argument depends on her skill in choosing just the right sources to document her case. She encases these key sources with enough material to make her interpretations generally compelling, although I found parts of the chapter on artistic "language" a bit forced, depending as it does on illustrative material from publications quite different from her central sources. She also expresses her argument with considerable assurance. Some of the several specialists with whom she politely disagrees may object that her argument is a bit too tidy. I predict that most of us, however, will find it unusually captivating and persuasive.

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MICHAEL GEHLER. *Studenten und Politik: Der Kampf um die Vorherrschaft an der Universität Innsbruck 1918–1938*. (Innsbrucker Forschungen zur Zeitgeschichte, number 6.) Innsbruck: Haymon. 1990. Pp. 591. S 485.

Michael Gehler's study of Innsbruck University students before the German Anschluss will complement the studies of German student involvement with National Socialism by Daniel Horn, Michael Steinberg, Michael Kater, Geoffrey Giles, Wolfgang Zorn, and Konrad Jarausch with an important Austrian dimension. This work is in fact the first full-scale study of an Austrian university's role in the Nazi triumph.

Gehler concentrates, as did Steinberg, on the economic, social, and political conditions that brought students to support the National Socialists. But Gehler's task is made more difficult because of the complex Austrian historical situation that divided students among strong competing Catholic, national, Burschenschaften, and Nazi groups during the turbulent democratic (1919–34) and corporative (1934–38) periods in interwar Austria. Gehler begins with an excellent section on the social backgrounds of members of the student fraternities. Almost all students

belonged to fraternities because they provided comradeship and gave members better job possibilities through connections with an old boys network in state and society. Numerous tables establish the elitist social composition of the fraternities: most elite dueling fraternities, the Corps, were primarily the offspring of the nobility and property-owning *Bürgertum* and the Burschenschaften more from the educated *Bürgertum*. The data also establish the elitist nature of those studying medicine and law and the more *kleinbürgerlich* nature of the theology and philosophy students. The influx of German students, reaching about 50 percent between the late 1920s and 1933 when Germany imposed a one thousand Mark tax on any German studying in Austria, shifted student sentiment toward pro-Nazi student associations.

Gehler believes that the overwhelmingly bourgeois nature of the students explains their long-term anti-democratic attitudes and ultimately their support of National Socialism. The postwar collapse of the traditional order plus the economic problems led the bourgeoisie to fear a loss of status. The anti-democratic attitudes of university teachers and administrators only bolstered the students' reactionary nature. The fraternities provided students with a protected aristocratic environment that shielded them from the real economic and social problems of the outside world. The exaltation of military virtues, the condemnation of the Treaty of Versailles, and German national attitudes were kept alive in the fraternities by the aristocratic duel and its code of honor, the drinking ceremonies, and the hierarchical structure that inculcated proper thinking as one rose to the top.

Among the political questions considered, Gehler contends that anti-Semitism was more decisive than the Versailles peace treaty and the South Tyrol and Anschluss questions in shaping student attitudes. He argues that many students built their political views around an anti-Semitic *Weltanschauung* despite the fact that less than 1 percent of the students at Innsbruck were Jewish. He traces student anti-Semitism to the prewar racism of Georg von Schönerer and the Christian Socialist movement. Anti-Semitic students feared that Innsbruck University would experience a large influx of Jewish students as had Vienna and Graz. Anti-Semitism increased as the economy worsened during the world economic collapse after 1929, and students feared they would not find professional positions. Therefore, the radical anti-Semitism of the National Socialist student organization (the Nationalsozialistischer Deutscher Studentenbund) was anticipated by the student fraternities. Students heatedly opposed the "Versailles System" since it severed the South Tyrol from Austria, prevented Anschluss with Germany, and worsened the students' unsatisfactory situation by exacting reparations payments from an already economically weakened country.

Gehler devotes a large portion of his work to the struggles among the various student groups for pre-

eminence at the university. Before the Nazi student association gained strength in the early 1930s, the battle was between the Catholic and nationalist student fraternities. Efforts to bridge the gap between them always failed and led to greater radicalization. After 1932 student corporations made up primarily of students from Germany brought increased support for National Socialism and increased turmoil to the university. After Catholic fraternities agitated and gained the dissolution of the German Student Federation in 1933, the national and Burschenschaften fraternities identified increasingly with National Socialist Germany.

This work is important for explaining the early Austrian support for National Socialist objectives among students and professors and for examining the economic and social reasons behind student attitudes. By revealing the roles that former fraternity members played in the Holocaust, the study also offers a more severe indictment of university students than does Giles's study of Hamburg students during the Third Reich, which established the failure of Nazi political education among students. More studies will perhaps find that Giles's Hamburg students were the exception rather than the rule. There are, however, some weaknesses in this study. Gehler explains that medical and law students were heavily involved in promoting Nazi goals but views their support only as a result of their bourgeois backgrounds. He argues that the duel played a role in forming student attitudes but does not elaborate concerning the role that ritual played in fraternity life. Despite these few drawbacks, this is an important work in our continuing effort to comprehend student attitudes and the triumph of National Socialism.

ROBERT WEGS
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HANNS GROSS. *Rome in the Age of Enlightenment: The Post-Tridentine Syndrome and the Ancien Regime*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1990. Pp. x, 411. \$54.50.

In this survey of Rome's history from the late seventeenth century to the French Revolution, Hanns Gross seeks to comprehend the combination of internal and external crises that transformed the city, materially and intellectually, in the course of the century.

Despite its unpropitious location, its ramshackle governmental structures, and a population notorious for idleness and corruption, Rome in 1700 was still one of the centers of European culture, and the papacy was still a European political power of some importance. In the next decades, the city suffered "a gradual loss of energy and integrative force" that Gross calls "the Post-Tridentine Syndrome" (p. ix). Even before the French Revolution shattered Rome's

ancien regime, the city was in danger of becoming politically and culturally irrelevant.

It was not through lack of effort. Papal governments took all sorts of initiatives in the eighteenth century: to reform finances; to improve agriculture and commerce; to preserve the archaeological heritage; and to promote the discussion of new intellectual and theological trends. In every case, the results were disappointing.

Part of the problem lay with the fact that each pope was simultaneously functioning as the spiritual head of Roman Christendom, as the ruler of a petty central Italian state, and as the godfather of an extensive and avaricious family. As Italy was drawn into the series of diplomatic and military confrontations that culminated in the war with France, it is no wonder that the popes were unable to maintain a consistent and effective policy.

The book is organized topically, so that one sees how the general loss of spiritual confidence and intellectual vigor affected every aspect of experience. One theme, introduced in an early chapter and elaborated later, is that of Rome's secularization. A city whose grand spaces and magnificent vistas were designed for the elevation of the piety of pilgrims came to cater instead to wealthy northerners in search of pagan artifacts.

Gross demonstrates that Rome's numerous poor were much better fed and sheltered than Paris's poor in the same era. Rome was "the capital of charity, of charity raised to a principle of government" (p. 196). The description of Rome's welfare system illustrates especially poignantly how traditional Christian values collided in the eighteenth century with population pressures, agricultural crisis, and fiscal irresponsibility to bring the state to the point of collapse.

While the first half of the book consists of a comprehensive examination of Rome's economic, social, and political institutions, the second half is a more topical treatment of the city's cultural and intellectual life. The result here is less satisfactory, although some topics, such as Rome's significance in the emergence of modern historiography, are very well done. The chapters on literature and art are useful and informative, but it may be that they try to include too much.

Overall, Gross maintains a nice balance between emphasizing Rome's distinctness and suggesting its importance as an example of the working out of larger eighteenth-century processes. Wide-ranging and well written, the book will be welcomed by specialists and non-specialists alike.

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OTTAVIA NICCOLI. *Prophecy and People in Renaissance Italy*. Translated by LYDIA G. COCHRANE. Princeton: Princeton University Press. 1990. Pp. xviii, 208. Cloth \$47.50, paper \$14.50.

Girolamo Savonarola was not alone. In the age of the Italian Wars (1494–1530), the Veneto in northern Italy, where Ottavia Niccoli's interest is centered, was suffused with prophets and prophecies. They documented the appearance of ominous "monsters," tried to account for such "horrors," and recommended devotional steps to stop them. In the early sixteenth century one of the main outlets for such wisdom was what Niccoli calls "popular" broadsheets, and it is this source of words and images that the author exploits to answer the question of whether this prophetic culture was located within the upper or official "level" of culture or at the level of "popular culture" in the cities. This is, in effect, to ask how such prophetic information circulated and what its social function was. In this "contribution to the history of communication" (p. xiv), Niccoli answers that there were indeed two distinct cultures of the elite and the "people" but that they were not isolated. The contemporary religious culture of northern Italy was "generalized" and circulated among these "levels." The pope, for reasons of political utility, borrowed from popular "folklore" and the people of the city, and, in at least one documented case, he also borrowed from high humanist tradition.

Niccoli presents the sometimes substantial, sometimes sketchy evidence to sustain these assertions in the form of some penetrating microstudies of particular occult or cunning phenomena from the time they appeared through their maximum European extension. The author does not claim to add much to social historical theory, and from a purely historiographic angle the grounds for Niccoli's demarcation of her period from that preceding 1494 and postdating 1530 (when prophecy was "feminized") are insufficient. Rather, following in the steps of several of her fellow Italians (Carlo Ginzburg, Adriano Prosperi, and others) and Donald Weinstein (*Savonarola and Florence* [1970]), what the author does offer is much new source material for Italian religious history in the early sixteenth century and a serious attempt to classify these "popular" idioms phenomenologically.

Chapter 1 is divided awkwardly between a first section on the vital statistics of prophecy during the Italian Wars (the nature of news circulation, the role of the singers of such news [*cantastorie*]), and a following section that describes certain prophecies from 1509–10. A chapter follows dealing with Niccoli's main interest, monsters: those particular malformed human and animal creatures that assumed social and political significance in this period, especially those appearing in Ravenna and Bologna. Chapter 3 studies contemporary apparitions, that is, the nocturnal appearances of the furious hordes over cities such as Bergamo. Next is a chapter on the apocalyptic teachings of Andrea Baura and Zaccheria da Fivizzano, including interesting information on the wandering preachers, often Augustinians, whom the author finds to have been particularly numerous at the time. The book then turns to the period after 1517. Chap-

ter 5 describes the scant Italian preaching of these years, when Luther was viewed as a monster. Chapter 6 examines the predictions throughout Europe of a flood to come in 1524 (it did not). The last chapter explores the theme of the apocalyptic Emperor, who turned out to be Charles V. According to Niccoli, the duration of this man's reign helps explain the decline in popular prophecy that set in after 1530, when misogynists, she says, increasingly dismissed prophecy as female.

Whatever one may make of these prognostications, this book will no doubt make the lived religion of the age more accessible to an English-language audience that persists in thinking that Italy had none.

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LILIANA FERRARI. *Una storia dell'Azione cattolica: Gli ordinamenti statutari da Pio XI a Pio XII*. (Dabar, Saggi di storia religiosa, number 9.) Genoa: Marietti. 1989. Pp. 252. L. 37,000.

Liliana Ferrari offers a constitutional history of Italian Catholic Action from its institution with the reforms of Pope Pius XI in 1923 to the postwar revision of the statute of the organization in 1946. Her main sources are the statutes themselves and the public explanations and explications to be found in papal speeches and the clerical press of the time.

The Catholic movement of the late nineteenth century consisted of a number of relatively self-governing lay Catholic organizations, linked by a loose structure. By the time that Pius XI ascended the papal throne in 1922, the Catholic movement was centered on the Partito Popolare Italiano and the Catholic trade unions, both of which were challenged by the rise to power of Mussolini's fascists in 1922. In 1923, Pius XI developed a new focus for the Catholic movement when he brought organizations for youth and university students together with those for women and men in a new structure, Italian Catholic Action, designed as a mass movement of the laity to assist the clergy in returning Italian society to the "Kingship of Christ" (p. 26). As defined in its statute of 1923, Catholic Action was a collection of lay organizations operating under the watchful eye of the clergy. The centralization proclaimed in the statute was tempered by the traditional independence of the separate branches of the organization and by the local particularisms of Italian society.

The statute of Catholic Action was changed in 1931, following a period of tension with the fascist regime; in 1940, after the outbreak of World War II; and in 1946, once that war was over. Ferrari discovers that each successive revision increased the degree of clerical control over the laity. In 1931, Catholic Action became an organization jointly operated by laity and clergy, but by 1940 the organization had been

totally "clericalized" (p. 109), its direction in the hands of a commission of cardinals. This clerical dominance continued in the revision of 1946.

Although branch organizations retained a degree of autonomy throughout this period, each revision enhanced the degree of control exercised by the national level over diocesan and parish levels. In theory Catholic Action operated with a diocesan structure after 1931, yet Ferrari shows that this was far from being the case. Centralization was enhanced in 1940 and, when the war was over and measures of centralization seemingly could be relaxed, the 1946 revision explicitly increased the authority of the central directorate.

Ferrari's thesis, which challenges conventional wisdom, is that the clericalization and centralization of Catholic Action was not occasioned only by conflicts with the fascist regime, by the outbreak of the war, nor even by the end of the war. Rather, Ferrari demonstrates convincingly that it was part of the continuing logic of Pius XI's original conception, continued by Pius XII, of Italian Catholic Action as the vehicle for returning Italian society to Christ. The clericalization and centralization were deliberately enhanced by the popes as opportunities arose.

This thesis, while appropriate for the fascist period, would presumably run into difficulty explaining developments after 1945, when Italy functioned as a more pluralistic and democratic society governed by a Catholic political party. The greatest weakness of this study, apart from a rather repetitive format for studying each statutory revision, is the way that it ends abruptly in 1946 without even mentioning, let alone assessing, the emergence of the Christian Democratic Party and the new context of the postwar period.

PETER C. KENT
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M. V. DMITRIEV. *Pravoslavie i Reformatsiia: Reformatsionnye dvizheniia v vostochnoslavianskikh zemliakh Rechi Pospolitoi vo vtoroi polovine XVI v.* [Orthodoxy and the Reformation: Reformation Movements in the Eastern Slavic Lands of the Polish-Lithuanian Commonwealth in the Second Half of the Sixteenth Century]. Moscow: Izdatel'stvo Moskovskogo Universiteta. 1990. Pp. 134. 65 k.

The Protestant Reformation, in the traditional view, met only with marginal success among the Eastern Slavs. Since the reign of Ivan III, Muscovy had formally integrated Orthodoxy into its state ideology and thus remained hostile to Protestant ideals. The volatile Polish-Lithuanian domain, however, proved more responsive to Protestant agitation: the Polish aristocracy, for example, for a while fell under the spell of the Socinian (anti-Trinitarian) movement. Both Catholic and Protestant zealots considered the unwitting participants of the unfolding confessional

drama, the Orthodox Ukrainian and Byelorussian population, as a legitimate target for proselytizing activity. Yet, insofar as the Orthodox stratum was concerned, the advent of Protestantism in Poland merely complicated its historical conflict with the Polish Catholic state. The Union of Brest (1596) and the subsequent course of events rendered Protestantism in the Ukraine and Byelorussia completely irrelevant.

M. V. Dmitriev examines the Protestant-Orthodox confrontation during the second half of the sixteenth century. He seeks to demonstrate that the Protestants perceived the Orthodox not as potential allies in the struggle against the Catholics but as bitter confessional antagonists. He also reviews the Protestant policies and tactics, examines the character and impact of the Protestant homilies and propaganda, and analyzes the effect of the Reformation on the subsequent political and cultural development of the Eastern Slavs. The treatment is chronological rather than topical and is divided somewhat arbitrarily into five chapters. Dmitriev establishes that the appeal of Protestant doctrines was both political and philosophical, particularly because of the moral and intellectual bankruptcy of the Orthodox church. Protestantism was supported mostly by the secular urban element, which wished to erode the "feudal" tendencies of the upper clergy. Progressive townspeople eventually organized themselves into lay confraternities. This secular element effectively exploited the vast array of Protestant philosophical and polemical literature to significantly reform the Orthodox church. In the end, the Reformation in the East-Slavic lands failed, for several reasons. Among these are: the fact that the Catholic-Orthodox struggle was more immediate and vital to the Eastern Slavs, while Protestant ideology lacked a purely local coloration; the Orthodox society was still emerging from its feudal stupor and did not have a large and politically conscious burgher class; the largely traditional and illiterate peasant masses failed to grasp the progressive character of the Reformation and saw in it yet another manifestation of the "Latin heresy"; and, finally, in the midst of the Slavic milieu, Protestantism ultimately proved too amorphous and esoteric as a movement for social and political change. Nevertheless, while Protestantism failed, it contributed to the cultural development of the Orthodox Slavs and proved instrumental in the rise of a civil society, which was to play an important role in the general revival of the seventeenth century.

Despite the unfortunate Marxist (or rather Engels) interpretation of the Protestant Reformation and the affinity—intolerably anachronistic nowadays—for ideological buzzwords that mar the text, this study has some merit. It is an adequate introduction to a complicated and often-neglected problem in historiography. Moreover, although somewhat ponderous in style and pedantic in interpretation, it is generally well argued and its conclusions prove fundamentally sound. The documentary base is quite impressive,

including significant primary sources. Judging from the tendency to discriminate in the actual utilization of the sources, however, one must assume that the views of certain established scholars, such as Orest Levitsky and Mykhailo Hrushevsky, still remain outside the pale of Soviet historical interpretation.

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D. PRODAN. *Problema iobăgiei în Transilvania 1700–1848* [The Problem of Serfdom in Transylvania, 1700–1848]. Bucharest: Editura Științifică și Enciclopedică. 1989. Pp. 398. 41 L.

Serfdom in Transylvania has preoccupied D. Prodan for over half a century. In that time he has produced massive studies of its development in the sixteenth and seventeenth centuries based on an exhaustive examination and analysis of archival sources. The present volume is of a somewhat different order; it traces the evolution of serfdom in the eighteenth and first half of the nineteenth centuries in broad outline and suggests directions that future detailed investigations should take. By the term *iobăgia*, the author means serfdom in general and peasant dependency in the German sense of *Untertänigkeit*. But, in the period before 1785, he is concerned with serfdom's most severe form, *șerbia* (personal servitude, *Leibeigenschaft*).

Prodan focuses his attention primarily on the regulation of landlord-serf relations, a long, drawn-out process that became a test of wills between the Habsburg court and the central bureaucracy in Vienna and the Magyar landholding nobility in Transylvania. Neither side deviated significantly from the position it had staked out in the first half of the eighteenth century. The court continually sought to interpose itself between the landlords and the serfs in order to enhance the latter's capacity to bear an increased share of public burdens and to reduce the incidence of their internal migration and flight abroad. For their part, the landlords never ceased to resist any measure that would diminish their control over the land and those who worked it.

Prodan dates the court's comprehensive approach to the complexities of serfdom from the middle of Maria Theresa's reign, but he also shows how noble opposition thwarted a regulatory breakthrough, as in 1769, when the *Certa Puncta* merely endorsed earlier half-measures. It remained for Joseph II to make the first major break with the past. Prodan's account of the circumstances leading to Joseph's abolition of personal servitude in 1785 offers an original assessment of the link between enlightened theory and agrarian reform. But neither Joseph nor his successors could overcome the resistance of the nobles to general legislation limiting the economic obligations of peasants toward their landlords, as is evident from Prodan's examination of neglected aspects of the

process of regulation between 1790 and 1811. He notes that through all of this travail, the peasants—the objects of so much contention—were allowed no part in the negotiations over their future. Yet, in his view, they exerted a decisive influence on events through their continued migration; their recourse to violence, notably a massive uprising in 1784, which hastened Joseph's decision to abolish personal servitude; and, finally, their aspiration to be free of all of the burdens of serfdom and to secure full property rights over the lands they tilled.

Prodan takes into account the new dimensions that the problem of serfdom assumed in the first half of the nineteenth century as Transylvanian society became increasingly complex. He calls attention to the fact that in the 1830s and 1840s "liberal" and "conservative" groups made serfdom a political issue, and a few persons even called its very existence into question. By then it had also become intertwined with the growing nationality conflict in the principality, since the majority of serfs were Romanian, whereas the landlords were mainly Magyar.

Prodan is careful to place his study of serfdom in a European context. In the final chapter he restates the position he has long held that serfdom in Transylvania was essentially an indigenous institution. He insists that it arose in the sixteenth century out of the peculiar conditions prevailing on the estates of nobles and not in response to production for the domestic or foreign market. By amassing striking evidence of the distinctiveness of agrarian relations in Transylvania in later centuries, he offers a formidable challenge to critics of this thesis.

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F. R. BRIDGE. *The Habsburg Monarchy among the Great Powers, 1815–1918*. Providence, R.I.: Berg; distributed by St. Martin's, New York. 1990. Pp. viii, 417. \$59.95.

SAMUEL R. WILLIAMSON, JR. *Austria-Hungary and the Origins of the First World War. (The Making of the Twentieth Century.)* New York: St. Martin's. 1991. Pp. xviii, 272. \$39.95.

An interesting and indeed remarkable feature of these two admirable studies of Austrian foreign policy is that they are so much alike in treatment and interpretation. Both authors are generally sympathetic to the Habsburg Monarchy and question the widely held assumption that its collapse was inevitable. Both quote the famous dictum of the Czech historian František Palacký that if the Habsburg Empire did not exist one would have to create it. "Imagine if you will," Palacký said in 1848, "Austria divided into a number of republics and miniature republics. What a welcome basis for a Russian universal monarchy." Both F. R. Bridge and Samuel R.

Williamson, Jr., agree that, because of the empire's inherent weakness, its survival depended on other powers' recognition of the validity of Palacký's observations. And both conclude that the empire's downfall was due largely to the breakdown of its international support, not to disruptive domestic factors.

In a sense, the Austria established after the Napoleonic wars was a creation of the other victorious powers, designed to preserve order in central and southern Europe and serve as a barrier to French—and Russian—ambitions. In 1849, Lord Palmerston used almost the same terms as Palacký in describing the monarchy's importance. Yet in 1914 Britain joined France and Russia in going to war against Austria, and in 1918 Britain and its allies were determined to wipe Austria, as an empire, off the map. A central theme of the books under review is to explain this dramatic reversal of fortune.

Bridge's volume is an extension in chronology and compression in coverage of his earlier work, *From Sadowa to Sarajevo* (1972), whereas Williamson takes up the story in detail with a discussion of the legacy of Alois Lexa von Aehrenthal. Both begin with excellent analyses of the position of the Habsburg Monarchy among the great powers, the Habsburg government, and the making of Habsburg foreign policy, and both go into considerable detail in describing how well this curious amalgam of peoples and institutions worked. "Loyalty to the existing order of things in Franz Joseph's dominions was the most marked characteristic of those who served the emperor in the foreign service (and in the Army and bureaucracy too)," Bridge writes. "It was a loyalty that paid no regard to social origins—and coherence of the service was in no way diminished by the steady increase in non-noble personnel (almost 50 percent by 1914). Above all, nationality meant absolutely nothing." Foreign policy was the concern of a small, supranational elite—the emperor, his close advisers, and their officials. "The influence of public opinion on its formation was negligible," a generalization Bridge subsequently qualifies at some length (pp. 17–18).

Both authors may be somewhat overgenerous in evaluating the positive qualities of the empire and the abilities of many of its statesmen. They are especially warm in their praise of Aehrenthal, although both are obliged to conclude that his most significant foreign policy move, the annexation of Bosnia-Herzegovina, was disastrous. That action ended Austria's brief détente with Russia (which depended in large part on Russia's involvement in East Asia in any case), converted Russia into a staunch supporter of Serbian irredentism, and generally stirred the flames of pan-Slav hostility. The appraisal of both authors of Austria's domestic and international situation in 1912 also seems to me excessively sanguine, but they are surely correct in saying that after 1912 Austria's international position declined sharply.

What went wrong? Almost everything. Russia had already been alienated by the Bosnian annexation,

and all Austrian efforts to regain Russian confidence and revive the sentiment of monarchical solidarity failed. Aehrenthal prided himself on having recovered the dubious loyalty of Italy, but his apparent success was due largely to Italy's involvement in war in North Africa (his colleagues muttered wryly that the Italian alliance was indeed dependable in being utterly undependable). At great cost to their relations with other powers, the Austrians realized a major goal of their foreign policy during the Balkan wars in establishing an independent Albanian state to block further Serbian expansion and Serbia's access to the sea, only to find the Italians trying to substitute Austrian influence in Albania with their own. Romania, another nominal ally, was being successfully wooed by Russia to forget its losses to Russia in Bessarabia and to focus instead on regaining the large Romanian population living under Austro-Hungarian rule in Transylvania. The monarchy's other ally, Germany, which had supported Austria with typically tactless ruthlessness during the Bosnian crisis, pursued its own political and economic interests in the Near East, frequently at Austria's expense, seemingly oblivious to the perilous nature of Austria's situation in the Balkans. Moreover, Germany was indirectly responsible for the overall decline in Austria's international position. William II's fleet-building program and bombast had driven Britain into ententes with France and Russia, which Britain could not afford to endanger by reviving its traditional support for Austria and the status quo in the Near East. Germany's alienation of France had also eliminated any prospect of French political or economic cooperation, at least so long as Austria remained in the German camp.

Meanwhile, the South Slavs and Romanians, backed up by Russia, pressed their advantage, driving Austria to the brink of desperation described in the famous Matscheko memorandum of June 24, 1914, which became the basis of Austria's appeal for German support following the assassinations at Sarajevo. That event is described briefly and clearly by both authors, with Williamson observing that William II's and Leopold Anton Berchthold's policies may have been influenced by their personal friendship for Franz Ferdinand, whom both had visited at his Konopischt estate a mere fortnight prior to Sarajevo. Bridge and Williamson are also generally in agreement about the course of the July crisis. Both regard Franz Xavier Conrad as a major bungler in his estimate about the relative strength of the Austrian and Russian armies and his direction of mobilization and military operations, and both describe at some length the quite incredible failure of the Austrian and German high commands to coordinate their strategy despite their recognition of the imminent possibility of war and the almost certain involvement of Russia.

Bridge emphasizes in conclusion that the Matscheko memorandum, drafted before Sarajevo, contained not the slightest hint of war but recommended instead a long-term effort to solve the South

Slav problem by patient diplomacy. Sarajevo radically transformed the situation, dramatically revealing South Slav irredentism as a mortal threat to the very existence of the monarchy as a great power. Bridge argues that the nationalist wrangles that paralyzed Austrian parliamentary life were merely squabbles within a political elite, and that the mass of the emperor's subjects would have been surprised to hear that the monarchy was "falling apart" in 1914, as many historians have assumed. "Indeed, it was to survive another four years of incomparably more serious crises, during which the Common Army—the sinews of the Monarchy—remained for the great part impeccably loyal." It was hardly the threat of imminent collapse that drove the monarchy to war in 1914, he says. That threat was an external one to the monarchy's prestige among the great powers. "In 1914 . . . war was a feasible option precisely because the domestic situation was not particularly serious" (Bridge, pp. 335–39). More historians are likely to agree with Williamson that external and domestic factors were inextricably jumbled, but he too emphasizes the monarchy's staying power and that, paradoxically, it lost its existence in proving that point. "In July 1914, desperation, blind faith, hope, ambition and exhaustion overcame prudence, experience and caution." The monarchy opted for a Draconian solution, with results that were equally Draconian and militarily devastating. "War brought not victory or a solution to the Slav problem, but rather defeat and dissolution. From those results momentous consequences still flow" (Williamson, pp. 215–16).

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LECH TRZECIAKOWSKI. *The Kulturkampf in Prussian Poland*. Translated by KATARZYNA KRETKOWSKA. (East European Monographs, number 283.) New York: East European Monographs; distributed by Columbia University Press. 1990. Pp. 223. \$39.50.

Kulturkampf w zaborze pruskim first appeared twenty years ago. Even in 1970 it might have seemed dated to Western scholars. Lech Trzeciakowski's historiographical introduction defines the issues narrowly, inexplicably ignores such indispensable works as Johannes B. Kissling's three volumes on the Kulturkampf (*Geschichte des Kulturkampfes im Deutschen Reiche* [1911–16]) and Helmut Neubach's study of the Polish expulsions of 1885 (*Die Ausweisungen von Polen und Juden aus Preussen 1885–86* [1967]), and treats Georg Franz's superficial narrative of the Kulturkampf (*Kulturkampf: Staat und Katholische Kirche in Mitteleuropa von der Säkularisation bis zum Abschluss des preussischen Kulturkampfes* [1954]), Ernst Engelberg's chapter on "1871 to 1896" in his survey of German history (*Deutschland von 1871 bis 1897* [1965]), and the contemporary account by the priest-activist Paul Majunke (*Geschichte des "Culturkampfes" in Preussen-*

Deutschland [1886]) as if they are of like kind and weight. Chapters narrating the outbreak of the Kulturkampf and the anticlerical legislation cover familiar ground. The remaining 40 percent of the book is devoted to Prussia's germanization policies and the Polish resistance, matters that Trzeciakowski relates only fitfully to the struggle between church and state that is his ostensible subject.

Trzeciakowski's real interest, however, lies not in the Kulturkampf but in the development of Polish national consciousness, whose weal or woe is the measure of all things that happened in the period. Public figures are evaluated according to their Polishness or lack thereof, with the criterion for Polishness apparently being neither language nor ethnicity, but commitment to the resurrection of a Polish state. Thus, Wojciech Haza-Radlic is referred to as "a partly germanized deputy of the Center" (p. 35), and Mieczysław Halka-Ledóchowski's resistance to the Kulturkampf is dismissed with the comment, "it was not a need to defend national values that made him engage in an open clash with the state; but a need to defend the rights of the Catholic church" (p. 74)—no vice, one might think, in an archbishop. Because of Trzeciakowski's difficulty in appreciating any motive other than nationalist ones, he cannot understand why liberal and ultramontane Poles let their quarrel with each other distract them from "the most important issue—the fight against germanization" (p. 142). The German-speaking majority in West and East Prussia, Poznań, and Silesia appear only as "the occupiers" (p. 152).

As an exemplar of a particular kind of liberal nationalist historiography, Trzeciakowski's book is interesting, and because he has used Polish and what were then East German archives, it might also have been valuable for its details. Unfortunately, the details cannot be relied on. The translation is miserable. A few examples: "Jesuits" (*jezuitów*) become "Jews" (p. 39); the dismissal of the Bishop of Breslau becomes "capital punishment" (p. 67); Ludwig Windthorst's quip, "*extra centrum nulla salus*," is rendered "No salvation with the Center" (p. 98); "gymnasium" (*gimnazjum*) is translated "grammar school" (pp. 128–32), which may not confuse British readers but will certainly make the analysis baffling to Americans. The editing is a disgrace. Lines left out, wrong names, and orthographic and syntactical howlers make reading a torture. If East European Monographs was not willing to pay a proofreader, it should not have published this book at all. Trzeciakowski is better served by his article, "The Prussian State and the Catholic Church in Prussian Poland 1871–1914" (*Slavic Review* 26 [1967]). A more modern and objective account of the Kulturkampf in Prussian Poland can be found in Zygmunt Zielinski's "Der Kulturkampf in der Provinz Posen" (*Historisches Jahrbuch* 101 [1981]), and of the germanization policy in Marjorie Lamberti's excellent book, *State, Society, and the Elementary School in Imperial Germany* (1989). Both topics are well-treated in Rich-

ard Blanke's articles and in his book, *Prussian Poland and the German Empire, 1871–1900* (1981).

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BARBARA JANCAR-WEBSTER. *Women and Revolution in Yugoslavia, 1941–1945*. (Women and Modern Revolution Series.) Denver: Arden. 1990. Pp. xvi, 245. \$26.50.

This volume is part of a series that seeks to examine the function of sex and gender systems in the revolutionary process and the extent to which women can overcome the limitations of political and cultural systems. Barbara Jancar-Webster, in this trenchant, well-written, and thoroughly researched book, willingly steps from the past into the present. The major part of the book deals with the participation of women throughout Yugoslavia in the army, the Yugoslav Communist Party, and the national liberation movement against the Germans; the last chapter focuses on the reverberations that the changed status of women in Yugoslavia have had on the contemporary generation. This added dimension makes this book particularly relevant for classroom use.

While one can argue whether it is essential for books on women to accept or to develop a particular typology of consciousness, as Jancar-Webster does here, the text of the work itself is solid and free from jargon. A brief introduction to the complex historical background of Yugoslavia dwells little on the ethnic issue of the area. Jancar-Webster's focus is on the participation of Yugoslav women in the war and the revolution, and she does not allow intra-Yugoslav frictions to impinge much on the story. Since the Communists were the ones who consciously, actively, and effectively recruited women into the fray, her focus on the party is justified, as much as one would like more information on the role of the women in the noncommunist camps.

In the new feminist movement of the 1970s in Yugoslavia and the disillusionment with communism, Jancar-Webster fears "an undervaluation of the gains made by Yugoslav women during the war, and an underestimation of the symbol of the Yugoslav woman partisan to the worldwide progress of women" (p. 187). Building on the gains made by their predecessors, the small group of new feminists in Yugoslavia are trying to push the discourse into a female and not solely male-determined mode. For Jancar-Webster, this provides proof of her earlier conclusion that an authoritarian revolutionary government can liberate women only if it understands the concept of autonomy of self. A communist authoritarian revolutionary movement that defined the role of women in predetermined terms can hardly be expected to understand the concern of women for autonomy and self-definition.

The question in Yugoslavia, and in all of Eastern Europe, is whether women today will be able to separate the genuine issues of feminism from the communist formulation of the "women's question" that has resulted in the double burden of women. Jancar-Webster maintains that the new feminists in Yugoslavia have understood the need to fight for institutional and attitudinal change, in much the same manner as groups have done in other societies. This book can certainly serve as a springboard for further study and discussion of this point.

Jancar-Webster has collected much first-hand information, based on questionnaires and interviews, to supplement the available rich material on the historical role of women in the period. Statistical data, translations of some documents, a glossary, an index, and interesting illustrations enhance this volume.

MARTHA BOHACHEVSKY-CHOMIAK
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LINDSEY HUGHES. *Sophia, Regent of Russia, 1657–1704*. New Haven: Yale University Press. 1990. Pp. xvii, 345. \$29.95.

Lindsey Hughes's book meets the standards of an old and honorable genre. Her work is a detailed study of the life, career, and times of Sophia, who ruled Russia in all but name from 1682 to 1689, based on a very careful, thorough analysis of primary sources, some of them unpublished, and the most important monographic studies of the period. Her analysis of foreigners' accounts of the period, the source of the most alluring anecdotes about Sophia, is particularly sound and helpful.

Since the author avoids the excesses of popular romanticized biography, her book provides comparatively little information about Sophia herself. After prudently dismissing the most flamboyant legends about the regent, such as speculation about the nature and duration of her personal relationship with Prince V. V. Golitsyn and her role in instigating the Moscow revolt of 1682, Hughes leaves the reader with a largely negative characterization of Sophia's personality, ideas, and policies: "we know rather less about certain aspects of Sophia's life than previous writers would have us believe" (p. 274).

The author's description and analysis of the revolts and coup d'état of 1682 illustrate her approach well. She narrates the events in chronological order, pausing to discuss the veracity of the most important sources on each of them. Her interpretation of the early phases of the revolt follows the central argument of V. I. Buganov's monograph (*Moskovskie vosstaniia kontsa XVII veka* [1969]): the *strel'tsy* revolted against the government first and foremost because they believed that they had been badly treated, not because competing court factions urged them on. She is careful, however, to avoid Buganov's tendency to romanticize the musketeers. In delineating her hero-

ine's role in these bloody events, she tends to dismiss or deemphasize the testimony of sources that credit Sophia with organizing the opposition to the accession of Peter I and stirring up the musketeers' revolt in the name of Peter's older half-brother, Ivan. Hughes has no doubt, however, that once the revolts ended and the system of dual rule was established, Sophia became the effective head of state and took the lead in turning back the challenge of the Old Believers and, later, in destroying the Khovanskiis.

As this treatment of the revolt of 1682 suggests, in Hughes's view the significance of Sophia's regency lies in the actions of her government and the extent to which she participated in them. After a detailed examination of governmental policy between 1682 and early 1689, the author concludes that Sophia was a remarkable woman playing a revolutionary role in Russian political life. In describing the regent's desperate struggle for political survival in 1689, she characterizes Sophia as "vigorous and determined, quite fearless about speaking in public . . . a thoroughly political animal" (p. 237). Other than Catherine II, Hughes concludes, she was "the most determined and capable woman ever to rule Russia" (p. 276).

In describing the regent's political role, the author chooses the middle ground. She argues—correctly, I believe—that Sophia should not be characterized as a radical reformer. As she puts it, "Sophia was more than a mere figurehead . . . but she was never a self-proclaimed reformer after the manner of her brother Peter" (p. 119). Moreover, an examination of Sophia's personal life and the cultural policies of her regime, Hughes's area of special expertise, reveals her to have been a person who attempted to integrate respect for Muscovite tradition "with receptiveness towards change in the form of closer political and cultural contacts with Europe" (p. 260).

Hughes has written the best study of Sophia's regency to date. The care and skill with which she has analyzed the sources and the clarity of her writing should make her work both helpful to the scholar and interesting to the general reader.

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ISABEL DE MADARIAGA. *Catherine the Great: A Short History*. New Haven: Yale University Press. 1990. Pp. viii, 240. \$25.00.

In the ongoing reevaluation of Russian history taking place in the Soviet Union, no ruler has generated more interest than Catherine II. Long derided as spectacle without substance, an instrument of the nobility who increased their hold on power during her reign, Catherine is being reassessed as a ruler. On the popular level, there is an even greater fascination with Catherine the person. The number of facsimile

editions of prerevolutionary works on Catherine and her reign published in the Soviet Union to satisfy this curiosity is large and growing. There are promises that soon the Soviet reader will even have access to translations of works on Catherine's era by contemporary Western historians.

Catherine has also been disparaged, if for different reasons, by many historians outside Russia. Her reassessment in the West began some time ago, a result of the significant amount of work done on her reign in recent decades, especially in Great Britain and the United States. This research formed the basis a decade ago for Isabel de Madariaga's masterly *Russia in the Age of Catherine the Great* (1981), likely to remain for some time the most important synthesis of work on that era. In the present volume, Madariaga shifts the focus from Catherine's Russia to Catherine herself, first as ruler and then as person. In so doing, Madariaga continues to draw on the current research of many of her colleagues.

The book is intended for the general reader. To the simple chronological approach of the first few chapters, Madariaga adds a thematic dimension in subsequent headings. As the patterns and continuities of Catherine's reign become clear by its middle and later decades, Madariaga introduces discussion of its educational, social, and economic policy. Every major historiographical issue in Catherine's reign is treated, deftly and judiciously.

Madariaga's Catherine is a sanguine ruler, secure within herself, confident of the appropriateness of her policies, active in their formulation, and never mystified or overly perplexed by the course of events. Scant attention is paid to the hypochondria depicted in a recent biography by J. T. Alexander (*Catherine the Great: Life and Legend* [1989]), or to Catherine's futile efforts for two or three years to make sense of the events of the French Revolution, a detail noted in a recent conference paper by D. M. Griffiths. As an unblushingly sympathetic portrayal of the significance of Catherine's reign in Russian history, Madariaga's book should quickly win its place on undergraduate reading lists.

It is unfortunate that the book was published without footnotes. This weakness is tempered by the annotated bibliography of recent studies, in which the assiduous reader can often locate the source of quotations within the text. When the paperback edition appears, perhaps at least the anonymous citations will be footnoted. That edition will also afford the opportunity to eliminate the book's few factual errors: Archbishop Ambrose was murdered in September 1771, not October (p. 47); Pugachev kept to the southeast, not the southwest (p. 64); the provincial reform law was issued on November 7, 1775, not November 2 (p. 67); the first Russian dictionary appeared in 1789, not 1788 (p. 99); Catherine's limitations on book publishing were promulgated in 1787, not 1797 (p. 116); and it was third-guild mem-

bers, not merchants generally, who could harness no more than one horse to a coach (p. 146).

If it is true, as Madariaga quotes N. M. Karamzin as writing (p. 211), that most Russians would rather have lived during the reign of Catherine than at any other time, then Catherine and her rule fully deserve a long look. The nonspecialist will find in this assessment sufficient reason to explain the fascination Catherine holds for contemporary Russians as well as the renewal of interest on the part of specialists.

GEORGE E. MUNRO
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S. V. MIRONENKO. *Stranitsy тайной истории самодержавия: Политическая история России первой половины XIX столетия* [Pages from the Secret History of the Autocracy: A Political History of Russia in the First Half of the Nineteenth Century]. Moscow: Mysl'. 1990. Pp. 235. 1 r.

In the conclusion to this short but quite meaty book, S. V. Mironenko points out its cautionary implications. The events related are of intrinsic historical interest, and they also carry a few important lessons: throughout the first half of the nineteenth century, the Russian imperial government—with the emperor in the lead—was not only aware of the necessity of bringing about political (constitutional) and social (serf emancipation) reforms but also undertook a number of bureaucratic efforts in this direction. All efforts failed, however, because of the stubborn, albeit passive, opposition of the top echelons of the administrative apparatus (*sanoynye chinovniki*), who did not wish to change their ways, open the path to social transformations, and endanger the prosperity and status of fellow nobles. Finally, Alexander II broke the vicious circle by enlisting “progressive society” in support of a “perestroika” and, in so doing, overcame the inertia of the bureaucracy and the opposition of the serfowners.

Mironenko illustrates these general conclusions by means of a comprehensive account of three episodes. First, to demonstrate Alexander I's commitment to political reform, he relates in detail the story of the drafting of the so-called Charter for the Russian Empire of 1820. Following his granting of a constitution to Poland and opening its *sejm* in 1819, Alexander had Count N. Novosil'tsev draft a “constitutional charter” for the empire (incidentally, along federal lines). The work proceeded in great secrecy, and the full text did not become known until it was found in Warsaw and published in the course of the Polish revolt of 1831. George Vernadsky gave a scholarly edition along with an extensive historical commentary in the early 1920s (*Gosudarstvennaia ustavnaia gramota rossiiskoi imperii 1820 goda* [1925]; French edition, *La Charte constitutionnelle de l'empire russe de l'an 1820* [1933]). Mironenko quite correctly stresses that this episode, which he summarizes fully

and which he puts in relation with parallel plans for peasant reform, shows Alexander I's abiding concern throughout his reign to introduce some form of constitutional order. He turned “reactionary” only after 1822 when it had become quite clear that he could not succeed without the help of society; but this would, in fact, have meant abrogating the autocracy.

Frustrated by this impasse, Alexander I yearned with renewed intensity to retire into private life; it explains his tergiversation as to the course he should pursue with respect to reform on the one hand, and in dealing with the spread of secret societies on the other. It is a significant factor, Mironenko believes, in accounting for the new order of succession agreed on between Alexander I and his brother Constantine in 1819, and in keeping the arrangement a state secret. So well kept was the secret that its beneficiary, Grand Duke Nicholas, did not know about it. As is well known, the succession imbroglio that followed on the unexpectedly sudden death of Alexander I in the Crimea in November 1825 (briefly Mironenko dismisses the legend of the emperor not dying then but withdrawing as a hermit to Siberia) triggered the Decembrist revolt of December 14, 1825. Mironenko gives a fascinating and well-documented account of the episode, stressing its peculiar aspects and the disastrous consequences of secrecy. In his opinion, the Decembrist uprising was an “insurrection that might not have occurred”—with what effect for subsequent history we shall never know—if the agreement in 1819 between Alexander I and Grand Duke Constantine on the order of succession had been publicized in due course. Mironenko, however, cannot explain (and presumably no one ever will) fully and adequately the motivations of the parties concerned.

The third episode relates, step by step, the organization and proceedings from 1839 to 1842 of one of several secret committees set up by Nicholas I to find a solution to the serf problem. In general terms, the committee's work, resulting in the “Law on obligated peasants” of 1842, has been known since the appearance of V. I. Semevskii's monograph, *Krest'ianskii vopros v Rossii v XVIII i pervoi polovine XIX veka* (The Peasant Question in Russia in the Eighteenth and First Half of the Nineteenth Centuries [1888]). But Mironenko adds much detail and offers a better picture of the clashes produced by the several solutions proposed by the high dignitaries who made up its membership. In putting together his lively account, Mironenko has judiciously drawn on papers scattered in several archives, some never used before. The image of Nicholas I that emerges from his account is much more positive than the one found in Soviet and general histories. Mironenko rightly emphasizes that the bureaucratic infighting was much more significant than divergences in economic interests and social policy.

The final episode, summarized briefly, deals with the last secret committee instituted by Alexander II to

solve the serf problem. Threatened with seeing a repeat performance of the debates and failure of the secret committee, Alexander abruptly took another approach: he stage-managed things so that the initiative for reform came from the local assemblies of the nobility (the so-called rescript to Governor General Nazimov and its follow-up in the provinces of European Russia). By letting the emancipation debate become public, the autocrat succeeded in overcoming bureaucratic opposition and inertia to achieve effective results. The relevance of this lesson for today's Soviet Union need not be spelled out.

As a good example of glasnost and of a move in the direction of genuine perestroika in the domain of Soviet historiography, the book is most welcome indeed. It would be captious to fault Mironenko for not fully shedding the vocabulary and some conceptual notions in which he has been trained and worked until now. The wealth of detail, solidly anchored in a vast array of archival documentation, is of interest to all historians. Greater familiarity with the works of Western scholars would have made him realize that his conclusions are not as original as he thinks. But he has put Soviet readers greatly in his debt by enabling them to look at nineteenth-century imperial politics and history in a refreshingly, for them, novel way.

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DAVID CHRISTIAN. *"Living Water": Vodka and Russian Society on the Eve of Emancipation*. New York: Clarendon Press of Oxford University Press. 1990. Pp. x, 447. \$74.00.

In this study, David Christian analyzes the sociology, economics, and politics of vodka in imperial Russia. Although not equally successful in all three endeavors, he has much to say of value. The heart of the work focuses on the monopolistic vodka tax farm that predominated in most of Russia between 1767 and 1863. We see the effects of the tax farm on drinking patterns, the quality of vodka, government revenues, technological innovation, class tensions, and patterns of resistance.

Christian's study is straightforward. He attributes the rise of modern drinking habits—individual, not communal, frequent rather than festive—to the increased monetization of the economy. But the monopolistic practices of the tax farm kept consumption and quality low and prices high and therefore were a fundamental impediment to modernization. In the end, corruption, consumer boycotts, and a growing faith in free trade forced the establishment of an excise system.

Christian makes a number of important observations that place his research in a larger context. Because of a monopoly in production, vodka was solely a cash commodity and did much to pull the peasantry into the cash economy. The sharp increase

in peasant disturbances in 1859, habitually treated by Soviet historians as a general assault on serfdom, was largely a response to substantial increases in vodka prices attendant to the recent auction of tax farms. And the same factor that motivated the abolition of serfdom, "just enough commitment from the autocrat to resist those . . . so adept at finding new reasons for delay," played a key role in the reform of the tax farming system (p. 369).

Having written on the whole a fine study, there is much imprecision in the author's thinking. The text is overlaid with a naive and unsophisticated Marxism, and a Marxist analysis is not consistently carried through. Consequently, the author's frequent recourse to Marxist economics takes on the appearance of textual oddity rather than insightful commentary.

In addition, the work is replete with lapses in statistical and economic analysis. "A real increase in the tax burden" and "in the redistributive power of liquor taxes" are asserted without taking into account the huge price inflation in eighteenth and nineteenth-century Russia (p. 193). Thus, Christian sees sharp increases where there may have been little or none. He alleges that distilling was an important "price support for all gentry grain-producers" while offering no evidence and citing a major study that he himself admits finds to the contrary (p. 223). His assumptions on the elasticity of demand for vodka are wholly speculative. Elsewhere, we are told that "government revenues in 1859 were significantly higher than those for the previous tax farm periods," only to be informed that the vodka "boycotts [of 1859] did in fact pose a serious threat to the Treasury" (pp. 317, 325). The taxation of vodka is viewed as "an exceptionally powerful instrument for the upward redistribution of wealth" (p. 373) without regard to how any tsarist (or even Soviet) government chose to expend its funds. The conclusion that "the abolition of tax farming forcibly diverted . . . capital away from a relatively unproductive sphere of investment and into more productive areas" is overstated (p. 374). Prior changes in the banking system and the elimination of legal restrictions on various forms of capital investment were of much greater significance. The suggestions that A. V. Chayanov's theory of peasant self-exploitation might be extended to the noble manor or that one cannot talk in terms of gentry profits from distilling are not well thought out.

A final note. In spite of Christian's often sober scholarship, it seems apparent he enjoyed undertaking research in Finland, a country renowned for its vodka consumption. While he offers much thanks to the staff of the Helsinki Public Library, only at the Slavonic Library of Helsinki University Library could he have consulted the materials necessary to produce this study.

STEVEN HOCH

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CHRISTINE D. WOROBEC. *Peasant Russia: Family and Community in the Post-Emancipation Period*. Princeton: Princeton University Press. 1991. Pp. xiv, 257. \$39.50.

Here is an outstanding volume that will be welcomed by historians of peasant society, the family, and women's studies. For decades, G. T. Robinson's *Rural Russia under the Old Regime* (1932) was the mainstay of Russian peasant studies. But Robinson's work did not encompass all aspects of peasant life. In particular, the family remained elusive. A decade ago, social historian Peter Czap wrote that we need to learn more about familial relations, the roles that married sons and brothers, mothers-in-law, and daughters-in-law played within the peasant household. In the intervening years, the field of Russian peasant studies, and with it the study of the peasant family, has burgeoned.

Christine D. Worobec's splendid book brings us closer to understanding the fiber of peasant society and the dynamics of the family. Basing her study on ethnographic sources, zemstvo statistics, judicial reports, and a comprehensive range of the latest Soviet and Western scholarship, Worobec treats the customs of property devolution, family life cycle and household structures, courtship, marriage, and the culture of patriarchy. Tables depict household size, variables relating to household fission, size of land allotments, kinship patterns, marriage age, and seasonal variations of marriage.

At the risk of presenting central Russian peasants as homogeneous, Worobec takes a broad approach, treating central Russia rather than any particular province or region. On the whole she is successful because, although she sees general cultural and economic patterns emerging, she is scrupulous in depicting regional differences and variations.

Worobec takes decisive, well-documented positions in the areas of historiographical controversy: was a large household the result of or the cause of peasant prosperity? She believes that the larger the household's work capacity, the greater its resultant landholding and the more secure its ultimate economic well being. Did the large, extended household or the nuclear family of married couples and young children dominate in the late nineteenth century? In an excellent chapter where she delineates regional differences and explores the many reasons for household fission, Worobec contends that while the simple nuclear family became more visible in post-emancipation rural Russia, the normative, extended family nonetheless prevailed. Although in areas of poor soil and heavy male out-migration, such as Yaroslavl and Kostroma, a relatively high incidence of household divisions between fathers and sons did exist in the 1870s, even here the nuclear family did not dominate.

In the controversy over the status and role of peasant women in the second half of the nineteenth century, Worobec is even-handed, concluding that it

is impossible to speak of the complete subordination of women or to regard them wholly as victims. Peasant women accommodated themselves to patriarchy and utilized power available to them. They ruled over daughters-in-law and watched out for the behavior of other village women. Although she tends to believe that wife-beating was generally accepted, she shows women bringing abusive husbands to court, even seeking and obtaining separation agreements, phenomena that seem to undermine assumptions that beating wives was acceptable. Unfortunately, due to source limitations, we are still no closer to knowing how representative these assertive women were. Our understanding of women's attitudes toward abuses remains subjective due to the lack of complete statistical data in township court records and the mixed message received from folklore. Nor do we fully comprehend family dynamics. Worobec's sources enable her to explore father-son generational friction, but little emerges about the nature of the mother-daughter bond over the long term, although hints of enduring closeness (for example court cases, which reveal a mother trying to protect her married daughter from the abuse of her husband and in-laws) are suggestive.

Worobec exploits existing sources to the fullest. Indeed, a strength of this important contribution to peasant studies lies in its imaginative blending of folklore and statistics. Her bibliographical essay, which discusses the problems in using each, will be helpful to future researchers.

BEATRICE FARNSWORTH
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DANIEL R. BROWER. *The Russian City between Tradition and Modernity, 1850-1900*. Berkeley and Los Angeles: University of California Press. 1990. Pp. xiv, 253. \$35.00.

Over the last fifteen years or so, many essays and monographs on Russian cities, have appeared principally by North American scholars. Using these studies and some Soviet archival material, Daniel R. Brower seeks to provide a synthesis of Russian urban history based on a "cultural approach." Urbanism, he argues, is an interplay of images of cities and actual practices. The city is the product of "meaningful actions, by which urban inhabitants make the city in some measure their own place" (p. 2).

During the eighteenth and early nineteenth centuries, some of the shapers of cities were imperial planners (especially Catherine II) with rational and regularized designs who thought of cities as centers of civilization. In the nineteenth century, administrators and civic activists, using European cities as models, sought to "modernize" Russia by disseminating literacy, education, culture, and patriotism to urban inhabitants in sanitized, industrialized, and technologically advanced cities. By mid-century, attempts to

shape cities through urban design were abandoned. Further administrative efforts went principally into policing migrants, who were variously perceived as vagrants, criminals, and hooligans.

The author concentrates his attention not on all Russian cities but on what he calls migrant cities: those with populations exceeding 15,000 people, of whom over half were born outside of the city. In this way he can reexamine the question of whether migration contributed to the "peasantization" of cities or to the process of integrating newcomers to urban life.

Among the many topics Brower considers are the advent of railroads, which facilitated the influx of large migrant populations, various statutes regulating municipal government, and the emergence of public opinion and civic activism outside the purview of the authorities.

While official Russia had a vision of an ideal Russian city, the migrants experienced urbanism. For them, the city was a workplace where they sought housing, suffered illnesses, and found companionship in taverns, recreation in collective fist fights, and amusement in the penny press. When labor conditions and social tensions became intolerable, they resorted to pogroms and riots. Such violence, he concludes, is intrinsic to the Russian city of the late nineteenth century.

Brower adopts "the methodology of urban history," which he defines only as "a strategy of illuminating historical understanding" (pp. 1-2). This methodology combines, it would seem, his cultural approach with some statistical analysis, but this is never stated explicitly. He uses data from the imperial census of 1897 to differentiate between "migrant cities," "intermediate cities," and "stagnant cities." The census is used only for two tables (pp. 78 and 234). The statistical analysis in the appendix is cursory and not well integrated into the book. It would have been useful to have included the correlation matrix that determined the choice of the "discriminating variables" used to establish the distinctive type of "migrant cities." The fifty-eight "migrant cities" are not listed by name so that one cannot assess the empirical significance of the author's argument for this abstract category. Statistical analysis is used sparingly elsewhere in the study, with only two tables (on the estate origins of Duma deputies in 1890 [pp. 109-10]) and a chart on the growth of urban schooling (p. 156).

On occasion the author's selection of topics is idiosyncratic: why discuss railroads and not industrialization, public readings and not popular theater, and the state's temperance movement and not that of private organizations? The role of the church in integrating migrants into cities is not considered. It is assumed that churches were only in city centers (p. 152) where people watched the "canonization of bishops" (p. 150). Presumably the author means consecration of bishops.

Because this "methodology of urban history" re-

mains vague, it is not clear that it will be useful to other urban historians. But the detailed essays by one of the foremost historians of Russian cities are a valuable contribution to the still slim literature available on Russian urbanism.

PATRICIA HERLIHY
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I. MICHAEL ARONSON. *Troubled Waters: The Origins of the 1881 Anti-Jewish Pogroms in Russia*. (Pitt Series in Russian and East European Studies, number 13.) Pittsburgh: University of Pittsburgh Press. 1990. Pp. xii, 286. \$39.95.

The pogroms that struck Russian Jews in the early 1880s are commonly acknowledged to have had momentous impact not only on Jewish history but on world history as well. The massive migration triggered by the pogroms created the basis both of the large American Jewish community and the *yishuv*, the modern Jewish community in Palestine. In Russia itself, the pogroms accelerated the flow of Jews into movements and parties opposed to the existing order and had dramatic influence on Jewish literary and cultural endeavors.

I. Michael Aronson has written an excellent book that addresses itself to just one aspect of the pogroms, its causes, and correctly demonstrates that the pogroms were not planned and led by the government or any other "hidden hand" but rather were the result of spontaneous actions taken by ordinary people as a consequence of a myriad of causes. There is little that is new here, the argument for spontaneity having been made before, but Aronson provides a greater amount of detail than is found in other works treating the subject.

It is in his analysis of the government's behavior that the author is at his best. The pogroms, he writes, caught the government by surprise and frightened it. The initial reaction, as evidenced by Tsar Alexander III's statement to Jewish representatives, was that the revolutionaries were behind the disorders. Shortly, however, leading officials became convinced that the revolutionaries did not incite the pogroms but could exploit them by turning the enraged workers and peasants away from the Jews and against the landed aristocracy and the government itself.

By the second half of 1881 the government was attempting to halt the violence. Officials concluded that the fundamental cause of the pogroms lay in Jewish economic exploitation of the worker and peasant masses. The belief that the Jews were the culprits flowed naturally from a long-standing anti-Jewish sentiment harbored by many bureaucrats at all levels. It was this hostility to the Jews that led some officials, including police and military officers, to respond mildly to the first pogroms confirming the belief in a government conspiracy. Aronson convincingly demolishes this hypothesis by revealing the govern-

ment's concern about the pogroms and its strenuous efforts to stop them, and by elucidating the tensions, conflicts, and ambiguities that existed between the civil and military realms that hampered effective action to arrest the "Southern storms."

The book, detailed as it is, lacks some material useful for enhancing the reader's understanding of events. The sections on causation would have benefited from a brief discussion of the role played by Fyodor Dostoyevsky in generating an anti-Jewish bias among government officials and educated Russians. The same could be said of men like A. S. Suvorin, whose *Novoe Vremia* published anti-Jewish articles from the middle of the 1870s. More important is the absence of a discussion in the chapter on the revolutionary socialists of the part played by P. Akselrod and M. Dragomanov, both of whom wrote much about Jews and the pogroms and were central to the deliberations taking place among the revolutionaries. On occasion, this fine book is marred by awkward writing.

These criticisms aside, Aronson has written a very good book. The argument for spontaneity and against conspiracy and planning is presented well. This study is a valuable addition to the literature on Russian Jewish history in general and the pogroms in particular.

STEPHEN M. BERK
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ROBERT B. MCKEAN. *St. Petersburg between the Revolutions: Workers and Revolutionaries, June 1907–February 1917*. New Haven: Yale University Press. 1990. Pp. xv, 606. \$45.00.

How serious a threat to imperial Russia's social stability was the labor unrest that troubled the capital city on the eve of World War I? Although Robert B. McKean's meticulous, exhaustive, and for the most part excellent study of "workers and revolutionaries" in St. Petersburg covers the ten-year period from 1907 to the February Revolution, the largest and most important section of the text examines the short but explosive prewar period (usually defined as April 1912–July 1914) and its background, and it is these chapters that are most likely to reignite the debate first sparked by Leopold H. Haimson's celebrated articles of 1964–65 (*Slavic Review*, vol. 23, no. 4; vol. 24, no. 1). Since the appearance of those articles, the social instability part of Haimson's thesis has been supported and elaborated by several specialists while continuing to provoke some barbed criticism from writers who resist taking it seriously, identify it with Communist historiography, and even go so far as to interpret the Petersburg unrest as a portent of progress and social peace. (A recent example is Richard Pipes, *The Russian Revolution* [1990], p. 192.)

McKean's study does not fit snugly into any camp, but I do see the weight of his evidence and argument

as falling on the side of social instability. That is, he treats the narrow, self-serving vision (or lack of vision) of Petersburg industrialists, the failure of imagination of government officials (especially police and military authorities), the resulting weakness and reduced appeal of trade unions and other labor organizations, and the growing militance of thousands of workers (especially metal workers) as indicators of regime instability and portents of systemic failure. Yet, at the same time, whether analyzing the prewar period or the war years, he insistently introduces "modifications" (following a generalization with the caution that it must be modified is one of McKean's favorite gestures) that would seem to distance him from some aspects of the instability thesis: the Petersburg experience was radically different from that of other industrial centers; labor unrest there was unevenly distributed by branch and neighborhood; although strikes were increasingly political, they were not seriously revolutionary; and, a point he comes back to repeatedly, neither the Bolsheviks nor other Left parties were able to provide the labor movement with effective city-wide leadership (in the period covered by the book). Related to the last point is McKean's insistence—which I find convincing, if excessively underscored—on the irrelevance of V. I. Lenin to events in St. Petersburg. It is not that individual Bolsheviks or other revolutionaries played no leadership role in the unrest but rather that the local leadership consisted primarily of lower-level cadres, often skilled and educated metal workers, who disdained or were indifferent to the lofty quarrels of émigré intellectuals. Although the book stops short of the post-February months of 1917, McKean's emphasis on lower-level militants is consistent with the studies of 1917 Petrograd by Alexander Rabinowitch.

Readers of McKean's book are certain to be impressed by the enormous amount of careful research that went into it. The range of his successful search for primary sources is extensive and includes a thorough probing of the State Historical Archives in both Moscow and St. Petersburg (TsGAOR and TsGIA), though not, to my surprise, the archive of the St. Petersburg region (LGIA). Memoirs, newspapers, magazines, and published documents are heavily used, and there are many generous as well as some critical (but mildly worded) allusions to the works of the other historians, mainly though not exclusively American, who have been working this territory over the past fifteen years (including some deserving but as yet unpublished Ph.D. theses). McKean's writing style is serviceable, if not very elegant, and his organizational scheme, essentially chronological with due attention to essential topics within each period, makes a long and detailed monograph fairly easy to read.

McKean has not closed the book on all the issues he has raised. Some historians whose conclusions he challenges (or "modifies") are sure to rejoin the debate. That debate, as I see it, should be a friendly

one, with disagreements mainly over nuance, emphasis, and phrasing, the kind of argument that revolves around full versus empty parts of a glass. On the still highly charged issue of the old regime's continued viability, however, the extreme "optimists" will find only tiny bits of solace in this well-balanced study.

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NEAR EAST

MAHMOOD IBRAHIM. *Merchant Capital and Islam*. Austin: University of Texas Press. 1990. Pp. x, 246. \$28.00.

The title of this book is somewhat misleading. It is not a general history or analysis of the relations between capitalism and Islam, but rather a detailed chronicle of pre-Islamic Arabia, the founding of Islam in Mecca and Medina, and the history of the early Arab empire to the end of the first civil war in 660. Mahmood Ibrahim's central theme is that merchant elites and merchant interests were the dominant forces in the founding of Islam and the Muslim empire.

The most important contribution of the book is an enlarged and refined treatment of a number of historical episodes. In chapter 3, on the pre-Islamic social structure of Mecca, the development of factional struggles among the resident clans and the rise to power of the Umayyad family are particularly well described. The stages in the creation of the merchant/client-based social structure built around Umayyad leadership is an important contribution. There is an equally detailed study of the economic provisions made for the immigrants who moved with Muhammad from Mecca to Medina.

Much of the rest of the book, however, while adding fresh detail, tells the story of early Islam in a generally familiar way. The account of the development of the Meccan economy, the differentiation of rich and poor, the breakdown of tribal society, and the role of Muhammad in articulating the problems of society and providing solutions for its social, economic, and political needs, makes up a fairly conventional view of the origins of Islam. The story of the expansion of the Islamic empire, here stressing how it served the interests of the Meccan merchant aristocracy and how it worked to integrate Middle Eastern merchant classes into a vast free-trade zone, clarifies but basically reaffirms the point of view established by M. A. Shaban in *Islamic History* (1971). So, too, the account of the early disputes within the Arab-Muslim aristocracy between the new segment (the people who acquired wealth and status as a result of their affiliation with Islam) and the traditional segments (the tribal and merchant aristocracy which derived its power from pre-Islamic conditions) retells a story already told by Shaban, and by Martin Hinds ("Kufan

Political Alignments and Their Background in the Mid-Seventh Century A.D." *International Journal of Middle East Studies* 2 [1971], 346–67; "The Murder of the Caliph 'Uthman" *International Journal of Middle East Studies* 3 [1972], 450–69).

Thus, while important in detail, the larger claim of the book to present a new theory of the development of Islam seems excessive. The theory that merchants and merchant capital were central to the social development of Arabia and Islam is not new. It was first presented by Henri Lammens and has been an underlying concept in the historiography ever since. Moreover, this theory, though well established, has been challenged. Patricia Crone has argued in *Meccan Trade and the Rise of Islam* (1987) that there is no evidence for Arabian participation in international trade; Ibrahim does not deal with her thesis at all.

Furthermore, his argument for the prominence of merchants in pre-Islamic Arabian society is not entirely persuasive. Ibrahim rightly describes the South Arabian societies, Qataban and Himyar, as based on kingly and temple institutions and as governed by an elite of landowners and tribal chieftains. He assumes that merchants are a part of this elite but presents no evidence to show that they owned capital or operated in a free-market economy rather than being a class of commission agents selling the surplus produce collected by kings, priests, and landowners. The distinction, as has been shown by Karl Polanyi, C. M. Arensberg, and H. W. Pearson (*Trade and Market in the Early Empires* [1957]), is critical to understanding the evolution of merchants and market economies. Ibrahim does not demonstrate that a pre-Islamic Arabian merchant class was differentiated from state, temple, and tribal elites.

The theory of merchants and merchant capital is offered to refute a number of alternative theories. The alternatives seem to be straw men: Arabia as a static, backward, and uncreative society; Islam as a bedouin religion. The real need here is not to replace single-perspective theories with another limited perspective, but to integrate the different views into a richer, more sophisticated concept of how bedouin and merchant groups, religious visions and economic interests, were related in the larger historical experience.

IRA M. LAPIDUS
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TAREQ Y. ISMAEL and RIFA'AT EL-SA'ID. *The Communist Movement in Egypt, 1920–1988*. (Contemporary Issues in the Middle East.) Syracuse, N.Y.: Syracuse University Press. 1990. Pp. xii, 218. \$37.50.

Rifa'at El-Sa'id is a prolific chronicler of the Egyptian communist movement. He has written nearly a dozen books on the subject in Arabic since the mid-1970s when, as part of the campaign to delegitimize the

regime of Gamal Abdel Nasser. Anwar Sadat instituted a limited liberalization that permitted Egyptians to begin publishing on such topics. The sheer volume of El-Sa'id's writing has rendered his version of the history of Egyptian communism the most prominent alternative to the Cold War, conspiratorial explanations of Egyptian (and Middle Eastern) communism as a Moscow-directed plot that were popularized by Walter Laqueur in works such as *Communism and Nationalism in the Middle East* (1956). El-Sa'id portrays the emergence of communism primarily in terms of Egypt's social and political conditions and the communists' ability to mobilize popular support by linking the issues of national and social liberation.

Tareq Y. Ismael's collaboration makes this approach accessible to an English-reading audience. The first two-thirds of the book summarizes El-Sa'id's previous work in Arabic and updates it with new information drawn from more recently discovered materials. In addition, this volume extends the history of Egyptian communism beyond the dissolution of the two communist parties in 1965, the final topic covered in El-Sa'id's most recent book on this subject, with an account of the reemergence of the Communist Party of Egypt (CPE) and three other communist organizations in the mid-1970s. The book's main sources are the public and internal publications of the communist organizations and interviews with former communist activists, which El-Sa'id has been assiduously collecting over the last two decades, and scattered court records of communist trials. Gathering these materials represents a considerable accomplishment, since none of the communist tendencies has an intact party archive and court records must be retrieved from the files of defense attorneys because the Legal Museum is not currently functioning.

El-Sa'id is a former member of the largest Egyptian communist tendency, the Democratic Movement for National Liberation (DMNL). Like his previous work, this book resembles an official party history reflecting the view of this tendency, to which El-Sa'id has been loyal throughout his career. It is primarily an organizational history with little to say about the social base of Egyptian communism, the experience of being a communist, or the concrete ways in which communists participated in social and political struggles. The difficult issues in the checkered history of the movement—the Alexandria general strike of 1924 that resulted in the suppression of the first Communist Party, the prominence of foreigners and Jews in reviving the movement in the 1930s and 1940s, the controversies surrounding DMNL founder Henri Curiel and the group of Egyptian Jewish émigrés he led in Paris in the 1950s, the leading role of elite intellectuals, the balance between class and national issues, relations between the communist factions, and the dissolution of the parties in 1965—are not problemized or treated critically. The authors make little effort to address recent public debates in Egypt or arguments by adherents of rival

communist tendencies and other historians that have proposed interpretations differing from their own.

JOEL BEININ
Stanford University

AFRICA

JANE I. GUYER, editor. *Feeding African Cities: Studies in Regional Social History*. (International African Library.) Bloomington: Indiana University Press, in association with the International African Institute, London. 1987. Pp. x, 249. \$29.95.

Africa's great cities sit amid relatively underdeveloped agrarian societies. How do these cities feed themselves? That is the central question of this volume.

The contributors to this project abjure standard approaches. Although one of them is a noted economist—Paul Mosley, writing on Harare—they seek to transcend standard economic analyses of supply, demand, and the formation of market prices. Another is an anthropologist, Jane I. Guyer, who edits the volume and writes a chapter on Yaoundé; yet the contributors seek to go beyond the analysis of class, gender, and ethnicity as determinants of economic behavior. Town against country, the market against the state—these and other simplifications find little support in this volume. Rather, the authors search out the *micro-level forces* that shape the provisioning of African cities and the local level organizations that structure market behavior.

Their approach is most clearly shaped by the anthropological interests of the editor and by the intellectual commitments of the geographers who contribute to this volume: Michael Watts, who writes on Kano, and Deborah Bryceson, who writes on Dar es Salaam. Inspired by francophone scholarship, the authors re-create in intensive detail the political, social, and economic interrelations that characterize the regions that provision the four cities studied in this volume and the historical paths that shaped their creation. The book emphasizes an *Annaliste*-like treatment of its subject. In doing so, it replaces earlier simplification with complexity. Much is gained, particularly a sense of time, place, and pattern. Something, too, is lost: a sense of causal structure.

Guyer's introduction provides a superb overview of contemporary scholarship, addressing works in history, economics, political science, and other disciplines. The case studies evidence a high level of scholarship. This is a serious and important work that makes a significant contribution.

ROBERT H. BATES
Duke University

ANDERS BJØRKELO. *Prelude to the Mahdiyya: Peasants and Traders in the Shendi Region, 1821–1885*. (African Studies Series, number 62.) New York: Cambridge University Press. 1989. Pp. xv, 192. \$44.50.

Historical studies on the nineteenth-century Sudan are few despite the fact that this period was the most decisive in the country's modern history. The Turko-Egyptian conquest of the Funj kingdom in 1821 marked the beginning of a distinctive epoch in Sudanese history, the first large-scale effort to draw the Nilotic region into the expanding capitalist economy. The main objective of the Turkish regime was the mobilization of the natural and human resources of the newly conquered territory through taxation and the transformation of agriculture and commerce. This volume, a welcome addition to the historical literature on this formative period of Sudanese history, examines the impact of Turko-Egyptian rule on the Ja'aliyyin people of the Shendi region in the northern Sudan. By combining accounts of European travelers, private papers of prominent Sudanese merchants, and oral literature, Anders Bjørkelo provides a thorough analysis of socioeconomic change among peasants and traders in this region.

The study is divided into six chapters. In the first three, the author provides an account of the Turko-Egyptian conquest of the region and describes the town of Shendi and the administration of the area after the conquest. The remaining chapters deal with the impact of the colonial economy on the local people. In this part of the book the author develops his main argument, that an alien economic policy with emphasis on cash crops and taxation of marketable commodities undermined the ecosystem, the subsistence peasant economy, and traditional society. Many peasants and traders responded by migrating to southern and western parts of the Sudan, which offered a variety of new options. Bjørkelo concludes that "the Turkiyya struck the final blow at the political and economic vitality of the Nile Valley north of Khartoum" (p. 147).

By using indigenous documentary sources and previously unknown manuscripts, this book adds a new dimension to the study of this period of Sudanese history. The study of socioeconomic change in the northern Sudan puts into perspective important developments in the history of the Nile Valley as a whole. These developments include the mass migration from northern Sudan to the Upper Nile region, the large-scale use of slaves in agriculture, and the Nilotic slave trade. The study has failed, however, to consider these developments as a climax of a process that began before the coming of the Turks. The dispersion of northern Sudanese traders and the use of slave labor had begun as early as the eighteenth century. Moreover, this work focuses on one region, one that cannot be taken as representative of the whole northern Sudan. Finally, the study seems to suggest that the response of the riverain people to the Mahdiyya was uniform, although the population of northern Sudan took different positions on the conflict. Despite these shortcomings, Bjørkelo's work is a valuable addition to the

historical literature on the first colonial period of Sudanese history.

AHMAD ALAWAD SIKAINGA
Harvard University

MOHAMMED HASSEN. *The Oromo of Ethiopia: A History, 1570–1860*. (African Studies Series, number 66.) New York: Cambridge University Press. 1990. Pp. xviii, 253.

Mohammed Hassen's book attempts to place the Oromo closer to the center stage of Ethiopia's public history. Yet his own scholarship reveals that the Oromo do not have a cohesive or corporate history. When the Oromo impinge on the public record, then Hassen can write about them; at other times he resorts to subjunctives, speculations, or assertions. Ironically, his book is largely based on published materials, the stuff of Ethiopian rather than Oromo history. He does use some oral sources, but he never explains how they were gathered nor how he judged them reliable.

Hassen begins by indicting the now classic study of Tadesse Tamrat, *Church and State in Ethiopia 1270–1527* (1972), for ignoring the Oromo as a factor in the history of highlands Ethiopia. He has uncovered "conclusive evidence which demonstrates beyond a shadow of a doubt that some settled agricultural Oromo groups lived in and to the south of . . . Shawa before the fourteenth century" (p. xii). His footnote at this point cites his own dissertation, but he subsequently clarifies by noting linguistic evidence that the Oromo were one of Ethiopia's primordial peoples: "To consider them as newcomers is a claim which has no historical foundation whatsoever" (p. 2). He characterizes the Ur-Oromo as mixed farmers, some of whom became pastoralists when they moved to the lowlands. Never does he explain such a surprising development, rare in world history, but for support he cites Eike Haberland (*Galla Süd-Äthopiens* [1963], p. 772), who recounted unsubstantiated Boran and Guji Oromo traditions. In other words, Hassen has based his analysis on hearsay.

Why make such sweeping claims on the basis of such tenuous material? The answer lies in the politics of ethnic competition in Ethiopia. If the Oromo inhabited the country's central highlands before their historic invasions of the seventeenth century, then current politicians can characterize Christian Semitic speakers, who have long dominated modern Ethiopia, as colonialists. In the late nineteenth century Menilek II (r. 1889–1913) restored Solomonic rule—or conquered, depending on your politics—in areas usually presumed to have been overrun by the Oromo in the seventeenth century. If Hassen's interpretation of history is linked to a negative view of northern aggrandizement, then partisans can agitate for an independent Oromia or for a special place for the Oromo within Ethiopia.

Oddly enough, Hassen's sweeping claims take up only a small part of his book. His major contribution is an excellent account of the Oromo migrations of 1570–1600, from Bale and Borana into Ethiopia's southern highlands. He also provides a well-documented and well-conceived history of the Oromo Gibe states, although there is little new analysis. Hassen is content to conform to the usual materialist notion that property and trade led to social differentiation among the hitherto egalitarian Oromo and undermined the governing age-grade system (*gada*). He seems to blame sedentary agriculture for the social loss.

While Hassen manages to avoid the issue of who owned the land the Oromo took, he cannot get around the fact that they enserfed the people they conquered, a precedent Menilek followed several hundred years later. The author also chooses to ignore the extent of Oromo cultural assimilation into the northern, Christian power structure during the eighteenth and nineteenth centuries; that would come too close to admitting that Ethiopia might possibly have a national history. Finally, the very strange organization and nature of this book—one chapter on migrations and five on the Gibe states—reveals the limited nature of Oromo history. Hassen nowhere explains that the Oromo are the country's most numerous and widely dispersed people, present in at least twelve clusters in ten provinces.

HAROLD G. MARCUS
Michigan State University

WILLIAM J. SAMARIN. *The Black Man's Burden: African Colonial Labor on the Congo and Ubangi Rivers, 1880–1900*. (African Modernization and Development Series.) Boulder, Colo.: Westview. 1989. Pp. xii, 276. \$45.00.

William J. Samarin is a linguistic anthropologist who has made a pioneering contribution to the history of Central Africa. This book is an inquiry into early European colonial expansion that asks, "How did so few come to dominate so many over such a large area in such a short time." His answer is emphatic. They mobilized thousands of workers, the vast majority of whom were drawn from the very areas they were intent on colonizing. Samarin documents again and again the extent to which the French and Belgian colonial enterprises were dependent on African labor as well as exactly how fragile the European position was in these first, crucial years of their penetration into the Zaire basin.

The work begins logically and chronologically in Europe, with an interesting discussion on the ideology of work and its role in justifying and promoting the "civilizing" enterprise of colonization. There follow roughly chronological chapters dealing with initial labor recruitment: the use of West Africans and other foreigners by early expeditions, the beginnings

of recruitment among Congo River peoples, and the use of freed slaves. The rest of the work is more topically organized, with chapters treating the use of "freed" slaves; the organization of overland porterage services; the development of riverine transport; the role of Christian missions (Protestant, Belgian Catholic, and French Catholic) in creating a colonial labor force; and on women as workers. Throughout the author draws provocatively on his linguistic background to explore not only the creation of a new kind of work force but also the role of ideas, language, stereotypes, propaganda, and communications in the process.

This is an important book, one that is full of insights and new information. It is not, however, an easy book to read, nor is it without significant flaws. Both the coverage and the documentation are uneven. It is apparent that Samarin has specialized in the riverine areas north of modern Kinshasa. The sophistication and depth of his analysis is noticeably greater for this area than for the lower river section between Kinshasa and the Atlantic. The extent of documentation for the former is also considerably greater. Although this uneven treatment weakens in particular his regional comparisons, it is justifiable on the grounds that colonization of the upper Zaire (Congo) and Ubangi rivers was a part of the race to control the entire Congo basin. The book also suffers another kind of unevenness that arises from a too slavish following of the sources and the unsystematic inclusion of too much raw data in the text. Last, Samarin's fondness for parenthetical comments coupled with the use of social science-style citations makes the text generally difficult to follow.

Despite its problems this is an important work, one that should be read by historians interested in better understanding the complex processes by which European powers established their hegemony over African ones in the late nineteenth century. It could also prove useful to comparative colonialists, labor historians, and those interested in methods of imperial expansion generally.

SUSAN HERLIN BROADHEAD
University of Louisville

JANET G. VAILLANT. *Black, French, and African: A Life of Léopold Sédar Senghor*. Cambridge: Harvard University Press. 1990. Pp. xii, 388. \$29.95.

This is a mature work of scholarship, crafted over nearly twenty years. It is a vintage study of a major African thinker and politician, Léopold Sédar Senghor, who served as president of Senegal for two decades. Janet G. Vaillant seeks to capture the nature of Senghor's mind and intellectual output by tracing his rise to prominence from an obscure Senegalese village to election as president of his nation, exploring along the way his formative years in Dakar and Paris, his development of *Négritude* as a major philosophical

statement, his activity in the French Parliament, and Senegal's struggle for independence. In her major emphasis—to search and analyze the mind and motivations of Senghor, particularly within the context of France—she has succeeded brilliantly. In her minor emphasis—Senghor's move into practical politics—the work is somewhat less successful.

Vaillant should be commended for undertaking a reassessment of Senghor. There are other works on Senghor in French and English, but none to my knowledge approaches Senghor with such a steady eye and a gift for interpretation. I have read many of Senghor's works and many of his commentaries, yet for me Vaillant is able to present a fresh perspective: she puts his literary works into context, explores his evolution as an intellectual, and digs into the ambiguities of his life under the Third and Fourth republics. She performs a service in tracking down much original material about Senghor's childhood and formative years and putting it into a comprehensive narrative. The chapters on Senghor's internment by the Nazis, his war years in Paris, and the dilemma he faced after the war—to make his career within the French higher education establishment or to accept Lamine Guèye's invitation to enter African politics—are outstanding. Vaillant's understanding of the French milieu in which Senghor operated until Senegal became independent in 1959 enhances her analysis of why Senghor slowly emphasized politics over poetry.

It is when Senghor finally becomes president of Senegal that we see the significance of the word order in the book's title: *Black, French, and African*. For many Africanists, a book on Senghor should be entitled *Black, African, and French*. I am among those who think that Senghor, despite his inauguration into the Académie Française, is first and foremost an African. But clearly Vaillant has set out to write a book that examines Senghor as a black Frenchman, and that is her prerogative. Her perspective therefore is laudable as long as Senghor remains in France, but it contributes to the blandness of the book's final chapters, covering Senghor's activity as president. This suggests that the story of Senghor in Africa, after he returned in 1946 to seek the position of deputy from Senegal to the national assembly may await another biographer. Vaillant's account of 1946 to 1959 is clear and helpful, but the assessment of Senghor's presidential years to 1980 skims the surface. She is still focusing on Senghor the intellectual, and does so very well, but in these pages Senghor the politician does not always emerge as convincingly as the intellectual. Vaillant lays the foundation for a broader political assessment, but then backs away from it.

To survive in Senegal, Senghor had to go beyond his intellectual achievements in France and call on all the shrewdness of his Serere ancestors. In an interview with him in 1972, I asked Senghor if it were true that he arose early every morning to work on his poetry. He replied yes, but "I also study my Wolof

lessons." The president wanted to improve his ability to communicate in the political lingua franca of Senegal. Vaillant does not give enough space to important secondary persons in Senghor's struggle upward—we would like to know more about Guèye, Mamadou Dia, Ibrahima Seydou N'Daw, and Senghor's wives. An exception to my reservation on the handling of local Senegalese politics is Vaillant's explanation of the split between Senghor and Mamadou Dia, the best account to date in any language.

This volume should be well received for its intelligent interpretation of Senghor as one of the major Third World intellectuals of our century, for its wealth of original sources, and for its judicious insights into the character of a courageous thinker whose originality, humaneness, and universality should have merited him a Nobel Prize years ago.

G. WESLEY JOHNSON
Brigham Young University

ELIAS C. MANDALA. *Work and Control in a Peasant Economy: A History of the Lower Tchiri Valley in Malawi, 1859–1960*. Madison: University of Wisconsin Press. 1990. Pp. xxi, 402. Cloth \$49.50, paper \$22.50.

To the uninitiated, the humid, poverty-stricken Lower Shire Valley of southern Malawi appears to epitomize the timelessness of Africa. Yet, as Elias C. Mandala demonstrates in this intelligent, thought-provoking study, there can be few rural areas on the continent that have been exposed to such a wide variety of economic experiences. Within the space of a century, the peoples of the valley were subjected to the full rigors of the slave trade, involved themselves in "legitimate" trade in ivory and oilseeds, sabotaged the attempt by European settlers to develop cotton plantations, pioneered the production of peasant-produced cotton, migrated in large numbers to the farms and mines of southern Africa, and participated in commercial fishing and cattle-keeping. In the last few years, in a development too recent to be discussed here, they have been exposed to the immigration of several hundred thousand refugees fleeing from the war in Mozambique.

Mandala brings to his work three qualities that are more often than not found in isolation: a mastery of the relevant archival sources, particularly for the earlier period; an intimate firsthand knowledge of the area cemented by extensive oral research; and a sensitivity to a wide range of theoretical issues relating to modes of production and to gender and generational relationships.

The first section provides an incisive, controversial account of the precolonial political economy of the area. Here, as in the rest of the book, Mandala gives due weight to the environment of the Lower Shire Valley (conditioned by fluctuations in the height of the river that influenced the extent to which floodland as opposed to dryland agriculture could be

practiced). At the same time, Mandala demonstrates "the ways in which the Mang'anja and Sena inhabitants domesticated and humanized the imperialist and ecological pressures for change" (p. 9). The discussion of the changing roles played by youth, slaves, and male and female elders in the various branches of the Mang'anja economy is particularly impressive. But there is also much to be learned, and not only by specialist historians of Malawi, from the accounts provided of African resistance to forced incorporation in the settler economy and of the changing role of women, in particular the ability of women to profit from the expansion of peasant cash-crop agriculture during the 1920s.

Although full of interest, the book is not without its limitations. Little insight is provided into the background of the *zunde* holders (wealthy peasants) who came to play a leading role in the cotton industry in the 1920s. The relationship between this group and the cattle-holding elite of the 1950s is assumed rather than demonstrated. Much of the material on developments from the 1940s onward is sketchy and underresearched. I personally remain unconvinced by the argument that prior to the expansion of the slave trade in the 1860s Mang'anja women were the social equals of men.

To comment on weaknesses, however, is not to dispute the overall value of this study. Mandala has written a work of genuine distinction. Although quite different in style from Landeg White's *Magomero: Portrait of an African Village* (1987), which focuses on social and economic changes in the highlands to the east of the Shire Valley, Mandala's book is similar in the richness of insights it provides. One minor irritation concerns Mandala's individualistic spelling of proper names—"Tchiri" for Shire, "Zembesi" for Zambesi, "Thete" for Tete—presumably on the assumption that for a name to be "correct" it has to be spelled phonetically. That is a consideration with which politicians may wish to concern themselves; historians as able as Mandala have better things to do.

JOHN MCCrackEN
University of Stirling

R. L. WATSON. *The Slave Question: Liberty and Property in South Africa*. Hanover, N.H.: University Press of New England. 1990. Pp. xi, 274. \$35.00.

This book on the antislavery movement in South Africa, although limited in scope, is a valuable addition to slavery studies, particularly as little has been published on slavery or on abolition and its impact in South Africa. R. L. Watson sets out to explain why no antislavery movement developed in South Africa comparable to those in Britain or the United States. Drawing on official documents, missionary archives, private papers, the press, and contemporary works, he outlines the views of officials, clergy, the business community, and farmers.

Most Cape whites did not believe that freedom was a basic human right or that slavery was sinful. Rather, they regarded property as sacred, slaves as legitimate property, and slavery as at worst a necessary evil. They also considered Cape slavery "mild" compared with West Indian slavery. Most groups saw it as in their interest to preserve the institution—slave owners in order to protect their property and keep control over labor; the clergy in order not to alienate their white congregations or the government; and the business community in order not to undermine the farmers. Even missionaries, although they might denounce slavery, did little to attack it directly. Missionaries had little commitment to slave populations: slaves were hard to convert and more likely to become Muslim than Christian.

As the abolition movement in Britain gained momentum, Cape colonists, both Afrikaner and British, merely sought ways to end slavery as gradually as possible to avoid economic or social disruption. The only antislavery organization, the Cape of Good Hope Philanthropic Society, limited its efforts to buying the freedom of "deserving" young female slaves who would become the mothers of a free working population. Some colonists thought slavery might be abolished in return for the establishment of a Cape Assembly, giving them greater control over their own affairs and presumably freedom to legislate their own labor laws. Ultimately the colonists' main concern was that slave owners should receive acceptable compensation for the loss of their slaves. On the vexed question of how far abolition and the lack of adequate compensation led to the Great Trek, Watson concludes that it was the last straw in a situation of rising fear. Stock farmers on the fringes of the Cape economy were convinced that British rule was undermining their way of life.

Watson discusses briefly why no antislavery movement developed in the British Caribbean. He also contrasts the weakness of the movement in the Cape, which produced no strong liberal tradition to counteract the development of systematic racial oppression leading to apartheid, with the vigorous abolitionist movement in the United States, which he sees as an antecedent to the civil rights movement.

Since this work is primarily a study of the attitudes of the vocal white population of the Cape, it necessarily tells us little about slavery itself or the results of abolition. It is, however, a welcome addition to a field in which there is room for much further research.

SUZANNE MIERS
Ohio University

COLIN NEWBURY. *The Diamond Ring: Business, Politics, and Precious Stones in South Africa, 1867–1947*. New York: Clarendon Press of Oxford University Press. 1989. Pp. xvi, 431. \$79.00.

The history of diamonds, from their production in South Africa to their sale to the rest of the world, is central to any serious understanding of the historical development of the entire African subcontinent from the 1870s. Although the diamond industry was to be dwarfed in economic importance after the 1880s by the development of gold mining on the Rand, it provided the initial impetus for what became the only industrial revolution in any part of Africa. Capital generated in the diamond sector played an important part in the capitalization of early gold mining ventures. Lessons learned by owners and managers of diamond mines about the recruiting and control of labor, including the notorious compound system, ultimately were employed in the gold fields. Two recent major works of social history, Robert V. Turrell's *Capital and Labour on the Kimberley Diamond Fields, 1871–1890* (1987), and William Worger's *South Africa's City of Diamonds: Mine Workers and Monopoly Capitalism in Kimberley, 1867–1895* (1987), have underlined this importance, but they did so by concentrating essentially on labor history.

Colin Newbury's major contribution is to fill out very handsomely the major issues of business history involved in these processes. He is concerned with unraveling the complex story of how the monopoly that emerged on the Kimberley diamond fields overcame the threat to its massive market influence implied in the opening up of new (and the expansion of older) sources of diamonds elsewhere in the world. He skillfully shows how that monopoly managed to outwit attempts by a number of national governments to control its power and subject it to the blast of competition. He demonstrates how the De Beers company in South Africa linked up with a complex series of European syndicates and ultimately protected its grip on the world market in gemstones even after the South African share of world production had fallen dramatically from almost 100 percent to about 20 percent. Newbury details how this unusual but all-important dovetailing between diamond retailers and diamond producers was achieved and how it worked in practice.

Newbury also effectively charts the relationship between governments and this monopoly. To the benefit of South African historiography, he demonstrates convincingly that government was often anything but the happy agent of mining capitalists. Although successive governments saw manifest advantages in the destruction of this powerful state within the state, such attempts persistently were stymied by an uncomfortable reality: whereas no government seriously considered outright confiscation, they did intervene to establish state ownership of some later-developed mines. Rather than weakening the grasp of the monopoly, this seems to have condemned the state itself to become part of the cartel by the twentieth century. This restriction of their room to maneuver was compounded by the importance of diamond-generated state revenues that made it haz-

ardous for even the populist-inclined politicians to do more than suggest reform of the system.

In his painstaking research through the dense and difficult thickets of official and business archives, Newbury provides a valuable and, sadly, rare insight into South African business history. The book is essential reading for anyone seeking to understand the development of the southern African economy after the 1880s.

RICHARD RATHBONE
Princeton University

H. PHILLIPS. *"Black October": The Impact of the Spanish Influenza Epidemic of 1918 on South Africa*. (Archives Year Book for South African History; Fifty-third year, volume 1.) Pretoria: Government Printer. 1990. Pp. xix, 281.

The influenza pandemic of 1918 killed some thirty million people in a matter of months, but, except at the local level, it has attracted relatively little attention from historians. H. Phillips's dissertation joins the company of Alfred Crosby's study of the United States (*America's Forgotten Pandemic: The Influenza of 1918* [1989]) and Geoffrey Rice's monograph on New Zealand (*Black November: The 1918 Influenza Epidemic in New Zealand* [1988]) as one of the few books dealing with the epidemic and its impact on a national scale.

This work is an almost entirely unrevised version of a doctoral thesis done for the University of Cape Town in 1984. An index has been provided, but the author has not considered more recent literature or attempted to place the South African experience in a wider context. Numerous Afrikaans quotations remain untranslated, a practice understandable enough in a South African dissertation, but frustrating in a work directed to a wider audience. Phillips provides almost no medical background on influenza epidemiology or on previous occurrences of the disease in South Africa. He does, however, present a graphic picture of the epidemic and thoughtful interpretations of its impact on South African demography, social and economic life, and politics. The impressive documentation includes a wide array of periodicals, newspapers, official and private archives, and dozens of reminiscences from and interviews with survivors. Phillips tries to give full attention to all ethnic groups, but the nature of the sources inevitably leads to a disproportionate emphasis on the white population and urban areas.

The first chapters vividly describe the epidemic and attempts to control it on the Rand, in Cape Town, Kimberley, and Bloemfontein, and in an African rural area, the Transkei. These places are well chosen to illustrate the variety of experiences and responses in a diverse country. Later chapters deal with public health and medical measures, popular responses, the demographic toll, and short and long-term social, economic, and political consequences. Not surpris-

ingly, despite much good will and cooperation, the epidemic unveiled hostility and suspicion between whites, Africans, and Coloureds, as well as tensions between Afrikaners and English-speaking whites. Africans frequently rejected white medicine or criticized official neglect; whites often blamed Africans for spreading the disease. Many Africans and Afrikaners sought to explain the disaster in religious or xenophobic terms.

Phillips's estimate of 150,000–300,000 deaths, roughly twice the official toll, is entirely plausible. Africans and Coloureds had higher death rates than whites. Natal, and hence the Indian population, suffered least. As elsewhere, the 1918 pandemic took an unusually heavy toll among young adults. Except for significant and long-lasting public health reforms and a short-term boom in life insurance sales, the epidemic mainly accelerated existing trends. These included moves toward a stronger welfare system, especially for poor whites, modest increases in government spending for African housing, and, at the same time, greater urban segregation.

Even in its unrevised state, this dissertation is a solid contribution to medical history and to the history of South Africa.

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JOHN E. SCHRECKER. *The Chinese Revolution in Historical Perspective*. New York: Praeger. 1991. Pp. xx, 240. Cloth \$45.00, paper \$17.95.

John E. Schrecker undertakes two related tasks. The first is "to bring the story of the nineteenth and twentieth centuries into accord with what might be called the traditional version of earlier Chinese history as portrayed by American specialists on that era and to draw the necessary consequences for the story of the revolution" (pp. xiii–xiv). This task is informed in turn by a more ambitious goal "to narrate and analyze the story in terms of traditional Chinese historiographical concepts, generally from the Confucian tradition," so as to rescue Chinese history from the hold on it of "Western theories" (p. xiv). The book's value rests on what the author does with the latter task, for it is too sketchy as a history to serve as anything but an illustration of the interpretive argument.

Schrecker attempts to rewrite Chinese history in terms of two concepts drawn from Confucian political theory: *fengjian* (akin but not identical to "feudalism," referring to the decentralized political arrangement that prevailed before the establishment of a bureaucratic monarchy in 221 B.C.), and *junxian* (usually translated as "commandery" or "the prefectural system," referring to the bureaucratization of local government following the establishment of the bureau-

cratic monarchy). A third concept, *datong* ("great unity," a utopian idea), enters the analysis in a more marginal way.

The book is divided into two parts of roughly equal length. The first part analyzes the three thousand years of Chinese history from the early Zhou Dynasty (ca. 1100 B.C.) to around A.D. 1800 with reference to *fengjian* (for the first thousand years) and *junxian* (for the next two thousand). The second part deals with the transformation and revolution that accompanied the systemic intrusion of the West in China after 1800.

It is puzzling why the author should have chosen this most conventional of the modern (corresponding to Western intrusion) and premodern (before the West) periodizations of Chinese history when his conceptual claims point in alternative directions, but the oversight is indicative of a general lack of rigor in the book's confrontation of concept and history. Schrecker takes an important step: to challenge the hegemony (my word) over Chinese historiography of Euro-American models of history (from around the turn of the century) that accompanied Euro-American political hegemony over China. Indeed, he seeks to turn the tables on the West by extending the Chinese concepts to the representation of European history. Why then does the book end up as a parody of what it sets out to criticize and, worse, reintroduce by the back door an even more atavistic form of hegemony?

Although critical of the domination of Chinese historical consciousness by "Western theories," Schrecker himself perpetuates the basic ideological premise of those theories that different histories may be inscribed within a single narrative (p. xvii). Similarly, seemingly unawares, "traditional" Chinese historiographical concepts appear in the analysis in a very untraditional guise, filled in with associations that are unmistakably modern. *Fengjian* and *junxian*, administrative terms in Confucian political theory, appear in Schrecker's analysis as something akin to Marxist notions of "mode of production" or "social formation" as conceived for European history. The author does not seem to realize that he can extend Chinese concepts to European history because, in the first place, he reinterprets those concepts from the perspective of European history. Thus, *fengjian* remains as feudalism (there is no clear account of differences here) while *junxian* assumes many characteristics of capitalism. Not surprisingly, since neither the concepts nor their relationship to Chinese history is rigorously analyzed, the interpretation of Chinese history that the book offers does not differ significantly from existing interpretations, unless new labels constitute reinterpretation in and of themselves. Likewise, the relationship of the present (the revolution) to the past takes the form not of concrete analysis but of culturalist reproach. The Communists, Schrecker writes, compounded the many concrete problems they faced "by separating themselves from

the viewpoints most appropriate to their needs: China's own traditions and, in particular, the radical thought of the *junxian* age and the 'socialist' theories embodied in the datong approach" (p. 166). Here is a Western oracle once again, telling the natives what to do with their past and future.

Schrecker takes it on himself to remind the Chinese that they remain "out of touch with the dynamic side of [their] traditions" (p. xix). That may be the reason that the book is almost completely oblivious to a century of Chinese effort to come to terms with the past and the historical scholarship of great diversity that the effort has produced. Apparently it takes a "Westerner" to get inside Chinese history because the crucial role that the West and Western ideas played in the revolution, in the author's words, "has tended to obscure China's indigenous background from both participants and historians" (p. xiii). Unfortunately Schrecker is not alone in this attitude. This book is a rather reckless example of a recent trend that has been fatuously described by its proponents as "Chinese history from the inside."

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CARNEY T. FISHER. *The Chosen One: Succession and Adoption in the Court of Ming Shizong*. (FEH/ASAA East Asia Series.) Boston: Allen and Unwin. 1990. Pp. x, 230.

The main subject of Carney T. Fisher's book is the Jiajing paternity dispute, the "Great Ritual Controversy" (*Dayili*) that arose in the early 1520s after the Jiajing emperor, Shizong (r. 1521–66), ascended the throne following the death of his cousin Wuzong (r. 1505–21), who had died childless. Shizong, inspired by the cardinal Confucian virtue of filial piety, sought to confer imperial honors on his natural father, Prince Xian, over the opposition of many metropolitan officials, who argued that the young emperor should instead venerate his imperial predecessor's father, Xiaozong (r. 1487–1505), as his adopted father, and relegate Xian to the status of imperial uncle. This dispute culminated in August 1524 in a dramatic public demonstration by more than 230 leading officials that Shizong forcibly suppressed, leaving "a legacy of distrust between throne and officialdom that would linger on for years" (p. 95).

That this ritual controversy aroused such strong passions at the time, preoccupying and polarizing officialdom and making and breaking careers, might mystify moderns. A possible Western analogue might be the Christological controversies of late antiquity that were also, in a way or in part, paternity disputes, and which had equally extensive ramifications. It is in the explanation of some of these ramifications, principally in the "interpretive commentary" of the third and final chapter, that the wider interest and significance of Fisher's book lies. Although the particulars

of the debate, as recounted in chapter 2 of the book, might seem tedious and trivial, the pivotal importance of the subject, ritual (*Li*), makes it a strategic point from which much of the political and intellectual world of imperial China might be assessed.

Because the participants in the paternity dispute invoked such diverse authorities as historical precedent, classical texts, and cosmological conceptions, Fisher finds the opportunity to include excursus on such various topics as Zhou dynasty clan law and the theory and practice of portents. Most importantly, he relates the two sides in the dispute to the two major schools of neo-Confucian philosophy, the Cheng-Zhu school (which found moral principles mainly in classical texts and historical tradition), and the Lu-Wang school (which found them in the mind). As the celebrated Qing scholar Huang Zongxi (1610–95) realized, "the Jiajing Ritual Controversy centred round a debate over the use of external models as opposed to intuition as a criterion for moral action" (p. 170).

In recounting and explaining the aspects and ramifications of this controversy, Fisher shows impressive erudition over a wide range of topics, although his presentation of these topics in the book's final chapter bears some resemblance to a collection of appendixes. Moreover, despite his considerable success in making sense of a debate that some earlier historians dismissed as either a trivial pursuit of mere names and forms or a transparent cover for a power struggle, Fisher himself occasionally offers anachronistic judgments on his subjects, as when he faults Ming scholar-officials for their "leech-like addiction to the past" (p. 6), for not anticipating the idea of progress, and for their "scholastic hair-splitting" (p. 147). Finally, one might question Fisher's characterization of Han scholars as "fundamentalists" who lacked critical acumen and philosophical astuteness (p. 26).

These minor criticisms should not, however, obscure the luster of Fisher's achievement in unraveling the intricacies and exploring the implications of the Great Ritual Controversy to reveal a measure of the mentality of Ming China.

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BENJAMIN A. ELMAN. *Classicism, Politics, and Kinship: The Ch'ang-chou School of New Text Confucianism in Late Imperial China*. Berkeley and Los Angeles: University of California Press. 1990. Pp. xxxiii, 409. \$45.00.

"New Text" Confucianism made its last great splash in China's public life in 1898, when the Cantonese scholar-official K'ang Yu-wei convinced the young Manchu emperor Kuang-hsu that Confucianism, stripped of most of its latter-day interpretive accretions, offered an inspirational guide to the complete political, scientific, economic, and social transformation of the country, sufficient not only to allow China

to catch up to Japan and the West but also to lead the whole world into the creation of a transcendent new order. That vision led to the sensational "Hundred Days Reform" in the summer of 1898. The reform failed. To date, it constitutes Confucianism's last major effort to make itself wholly relevant to the exigencies of modern civilization.

New Text Confucianism got that label not in 1898, but late in the first century B.C., in the Han dynasty. Ironically, in the first century B.C., New Text had come to stand for conservatism and the status quo. It was "Old Text" Confucianism that embodied the urge for thoroughgoing systemic reform. In the long run, Old Text Confucianism won out and became conservative in its turn. The Neo-Confucian movement of the Sung (eleventh century) and later side-stepped the controversy over the textual authenticity of the Confucian classics by emphasizing the so-called Four Books in their place. In so doing, Neo-Confucianism made its appeal on a philosophical level rather than on historical-classical or evidentiary grounds. So things stood until around the time of the collapse of the Ming dynasty in 1644.

Benjamin A. Elman's earlier book, *From Philosophy to Philology* (1984), detailed the story of the revival of evidentiary, mainly Old Text, Confucianism ("Han learning") in the seventeenth and eighteenth centuries. Behind the revival lay a sense that Neo-Confucianism ("Sung learning"), by imposing a rigid moral and political orthodoxy while at the same time encouraging divisive literati partisanship, had played an important part in the Ming dynasty's decline, and had thus reached a dead end.

The present book begins where the earlier book left off and takes the story into the late eighteenth and early nineteenth centuries. Elman argues that the scandalous rule of Ho-shen (the corrupt Manchu courtier who assumed de facto power in China from 1776 to 1799, during the Ch'ien-lung emperor's old age) occasioned what turned out to be a permanent loss of leadership and moral authority on the part of the throne, and forced some among the previously quiescent scholar-officials and literati to rethink the Confucian legacy. It was in this connection that Chuang Ts'un-yü (1719–88), who was a high official of the Board of Rites in Beijing, a Ch'ang-chou native, and an evidentiary scholar, rediscovered the long dormant vision of New Text Confucianism. Through his researches, he managed to set in motion a complex series of developments that over the next several generations produced significant reorientations in China's intellectual circles. It was no longer acceptable to be apolitical and dispassionate. Scholar-elites grew increasingly political and emotional. Inward-turned preoccupations with lineage faded, replaced by a new interest in forming political and study associations across lineage boundaries. The image of Confucius shifted. From a guardian angel of fixed and timeless systems, he was reinterpreted to become a creative innovator in the long-lost world of

his own time, and as such a model for action in the very different world of the early nineteenth century. (By the late nineteenth century, the New Text vision would end in what came near to being a religious messianism.)

Elman scrutinizes at great length a previously obscure stretch of intellectual history. He also shows how fundamental shifts in ideas were linked to political and social change. This book provides essential background for understanding just what lay behind and led up to the Reform of 1898. The book, however, is directed more to specialists than the general reading public. Six of the nine chapters describe in detail the slow evolution of New Text research on the Confucian classics, and these make for very hard reading.

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CARL A. TROCKI. *Opium and Empire: Chinese Society in Colonial Singapore, 1800–1910*. Ithaca: Cornell University Press. 1990. Pp. xiv, 260. \$39.95.

This important book raises some extremely challenging and provocative questions for those interested in comparative colonialism, the British empire, the Chinese overseas, and the modern history of Southeast Asia, especially the Malay world. On one level Carl A. Trocki's book is a detailed reconstruction and reinterpretation of the socioeconomic history of the Singapore Chinese in the nineteenth century, with a particular focus on the intra-Chinese violence and conflict accompanying the shift from more communal to more capitalistic forms of enterprise. These conflicts have been the subject of much heated scholarly debate, and Trocki strongly refutes the traditional explanations offered by many historians. On another level this book relates the socioeconomic evolution of the Singapore region to broader currents within the global economy of colonialism, in the process revealing the major role played by opium trafficking in that economy.

Trocki traces the history of the Chinese *kongsis* (cooperative enterprises) in the region. In contrast to most studies, he identifies a certain communitarian, working-class democracy in the early versions of these organizations. He also rejects the British colonialist theory that the Chinese secret societies interlinked with the *kongsis* should be seen as criminal organizations to be repressed, rather than as populist socioeconomic groups. But by the mid-nineteenth century the opium trade, expanding British power, and the rise of well-connected business interests who increasingly dominated Chinese institutional life were altering the nature of Chinese society in the direction of capitalist patterns, to the detriment of the working classes. Hence, Trocki believes that the endemic violence contained a strong element of class struggle, of a moral economy resisting commercialization, fueled

by local strains generated by the shift toward a capitalist world economy. By the early twentieth century the colonial state, in partnership with wealthy Chinese, had eliminated the egalitarian, *kongsi*-based economic patterns by coopting the former worker-brotherhoods.

The opium trade was critical to this transformation. Trocki argues that the British empire in Asia was essentially a huge drug cartel until well into the twentieth century, with Singapore and the nearby Malay states linked to a larger, British-dominated opium distribution network stretching from India to China. Indeed, British political and economic influence in Asia was closely linked to the spread of the opium trade. Trocki marshals considerable evidence to demonstrate that the opium "farms," leased to Chinese businessmen and trafficking to the Chinese working class in the Malay world, were the economic mainstay of colonial rule, facilitating exploitation of local Chinese addicts: "Any argument that the imperial system did not rely on opium and was not, in a pathogenic sense, systemically dependent on the drug is simply not in accordance with the facts" (p. 237).

Trocki is to be commended for an approach that involves sharing his evidence and reasoning, and he is careful to note where the sources are inadequate. There are some problems; for example, he seems occasionally to confuse the Teochew and Cantonese speech groups. Furthermore, Trocki's argument raises the essential points and is the most complete to date, but it is not fully convincing. Much of it is based on informed surmise and debatable assumptions about the early *kongsis*. But at least his book provides a challenge to those who will object to his conclusions and Marxian approach, as well as to those economic historians who have neglected the role of opium in modern Southeast Asian and colonial history. It is to be hoped that the book will reach as wide an audience as possible. Let the debate rage.

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WERNER MEISSNER, *Philosophy and Politics in China: The Controversy over Dialectical Materialism in the 1930s*. Translated by RICHARD MANN. Stanford: Stanford University Press. 1990. Pp. viii, 230. \$37.50.

Werner Meissner's work raises a crucial issue in the methodology of cross-cultural study. How do we categorize and explain the transplantation of a system of thought, in this case, dialectical materialism in China? The problem is not just that dialectical materialism changed but rather, that in the Chinese context, it changed into something quite different.

The texts of Chinese dialectical materialism, Meissner finds, "lacked a genuine philosophical content [when] compared . . . with the original works of Hegel and Marx." After much rethinking, he concludes

that they only made sense "as a self-contained system of analogous thought" (p. v). Western concepts such as "philosophy," "science," "materialism," "dialectic," and so forth, when translated into Chinese, lost their philosophical content. They became instead symbols in a sort of thinking that reminds him of "mythical and even totemistic thinking." In China, however, "the social base unit is no longer the tribe, but rather the party." Statements in Chinese dialectical materialist texts correspond to certain political opinions, which are in turn linked to party factions. "[P]hilosophical' organisation of the world is performed by authors according to intra-party struggles, and thus reflects the socio-political structure of the party" (p. 8).

Meissner concludes the Chinese Communist debate on "formal" and "dialectical logic" in the 1930s reflected "not the process of contemplating problems of logic, but rather the struggle over the United Front on which all the 'philosophical' statements were based" (p. 180). Furthermore, "the 'law of contradiction or the law of the unity of opposites' did not contain a general ontological statement, but was rather the military strategy of Mao Tse-tung couched in the language of philosophy" (p. 181).

Western philosophy, including dialectical materialism, is Meissner's norm. He is using a Western, logo-centered binary opposition—philosophy and its negative other—to categorize cross-cultural transformation. Thus, Chinese Communist writings on dialectical materialism are other than philosophy, whether mythic, symbolic, political, or military. Certainly dialectical materialism in China could never be considered a philosophy, and in this respect Meissner is correct. But I would argue it is important, in cross-cultural studies, to get away from Western-centered binary oppositions if we want to analyze properly the transformation of systems of thought in the recipient culture.

I suggest that instead of considering translated Chinese concepts as symbols (with the implication of being mythic and totemistic), we need to approach them as signs dislodged from a Western philosophical context, to be used for entirely new, different purposes. These signs no longer operate as chains of signifiers within a Western context. They are peripheralized (that is, dislodged from the Western context yet retaining the prestige of their Western genealogy), to become building blocks in the discursive construction of Chinese Communist realities. Furthermore, we have here an opportunity to revise the Foucauldian concept of discourse/power. By that I mean Chinese Communist discourses have greater power than Foucault's notion of localized bodily discipline, because they also aim directly to generate a national center for modernizing authority. In other words, we need to revise Western concepts of semiotics before using them in cross-cultural history.

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LINDA BENSON. *The Ili Rebellion: The Moslem Challenge to Chinese Authority in Xinjiang, 1944–1949*. Armonk, N.Y.: East Gate of M. E. Sharpe. 1990. Pp. xxvii, 265. \$45.00.

The history of the East Turkestan Republic (ETR), which controlled northern Xinjiang in the mid and late 1940s, has always been murky. Founded by military force in October 1944 near Yining, close by the Soviet border, and effectively terminated in August 1949 when its most important leaders perished (according to Soviet sources) in a plane crash at Alma-Ata, the ETR has often been portrayed, particularly by Chinese writers, as little more than a front for Moscow. Linda Benson challenges this interpretation in her important, but uneven, study.

Benson presents the most detailed account of the ETR yet published in English: a full political narrative, biographical information about officeholders, tables of organization, and much about economic and social conditions. It also treats extensively the Chinese Nationalist government's attempts to deal with the challenge of the ETR by reaching a genuine political accommodation in the region. On these subjects Benson will not soon be superseded.

But the account has weaknesses, both in use of sources and in analysis. For example, it draws heavily on Xinjiang newspaper collections and the archives of the Xinjiang provincial administration, both held in Taipei and mostly in Chinese. Yet the book's Chinese glossary is so full of errors as to raise doubts about the author's command of that language. Russian sources are few, outdated, and evidently used in translation (as are Turkish materials).

These defects are matched by problems with Benson's interpretation. Thus, arguing against those who have stressed the Soviet role in the ETR, Benson maintains that it was above all a manifestation of "twentieth-century nationalism" (p. 8). Yet she never tells us exactly what she means by that. Xinjiang, after all, is an artificial polity, whose people have cousins across its borders, which in any case were drawn by Chinese armies. Benson may therefore be correct that the inhabitants of Xinjiang do not legitimately belong to China. But does this make the territory of Xinjiang a nation, as she seems to assume? Surely the same political artificiality that argues against it as a province of China undermines Benson's vision of it as well.

Rather than a real "national liberation struggle" (p. 8), it seems more plausible to consider the creation of the ETR as an attempt by Moscow to establish a useful mock national republic, following roughly the same blueprint as the short-lived Far Eastern Republic in Siberia. Certainly Benson provides plenty of evidence for a major Soviet role in the ETR, in spite of her thesis. And Soviet sponsorship of the ETR would be entirely consistent with the pattern of Kremlin border policy in the 1940s, which sought to increase security, both directly through annexations and indirectly by fostering friendly, client, and pup-

pet regimes. Furthermore, that the impulse for the ETR should have come substantially from outside would follow the same historical patterns that saw much of the organization of the great rebellions of the nineteenth century done in what is now Soviet Central Asia.

Benson is correct that the genesis of the ETR was a complex business, and involved many people who had no intention of serving the Soviet Union's interests. But she almost certainly underestimates the importance of the Soviet hand. Rather than dealing with the Xinjiang story in isolation, then, Benson would have done better to place the ETR in a context that included Soviet moves in other border areas, including Mongolia, Korea, and above all Manchuria. Such reservations, however, should not substantially diminish her achievement. Books about Xinjiang, let alone books as good as this, are rare and therefore to be welcomed.

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HARISH KAPUR. *Distant Neighbours: China and Europe*. New York: Pinter; distributed by Columbia University Press. 1990. Pp. 231. \$47.50.

According to Harish Kapur, despite the People's Republic's more publicized concern regarding its relationships with the Soviet Union and the United States, Europe has nevertheless historically played a significant part in Beijing's foreign policy considerations. Within the various priorities that have motivated recent Chinese foreign policy (security, modernization, reunification, and promoting revolutions), Europe was often particularly important. Regarding security, a strong Europe offered a counterbalance to both excessive American and, later, Soviet strength. On the issue of modernization, Europeans East and West have consistently been more willing to consider transferring sensitive technology to Beijing. And, of course, for internal reasons Europe was more politically acceptable as a partner than, for example, the United States, which had been an enemy for so long. On the important question of reunification of China, none of the European nations was as committed to Taiwan as was the United States and could thus be more flexible. If help in promoting revolutions in the Third World was not an important advantage to having good relations with Europe, by the 1980s that issue had not survived as an important part of Beijing's priorities anyway.

On purely scholarly grounds there is little to criticize in this volume. The author uses his wide range of sources well and is thoroughly organized. The material is easy to follow and, especially important to practitioners, easy to abstract. Nevertheless, it is in tone far more a work of political science than the sort of study usually produced by historians. In fact, although Kapur offers this forty-year study of Chi-

nese foreign policy in a clearly chronological fashion (because the various stages of Chinese diplomacy were so clearly tied to specific historical moments), he actually seems apologetic about doing so. Moreover, most historians, usually sensitive to the nuances of the historical experience and of the lives of individuals, would probably have preferred that the work make a stronger effort to put Chinese foreign policy into a framework larger than merely the forty years emphasized by the author. It is almost as if Chinese foreign policy were born *sui generis* with no ties to the historical Sino-European relations of the past. The individual personalities of the Chinese leadership and their own experiences or lack of them in Europe are almost completely ignored. Surely the fact that some leaders (including Zhou Enlai and Deng Xiaoping) were quite familiar with Europe, having spent time there as young men, while others (such as Mao himself) had hardly ventured beyond China must be considered relevant to any understanding of the decision-making process among the Chinese leadership. Kapur, however, almost completely ignores the human element. In fact, at times Kapur's China seems more an abstraction than the world's most recent form of one of its most extraordinary civilizations.

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E. PATRICIA TSURUMI. *Factory Girls: Women in the Thread Mills of Meiji Japan*. Princeton: Princeton University Press. 1990. Pp. 215. \$29.95.

The book that has been crying out to be written in English has finally seen the light of day. E. Patricia Tsurumi's work, based on Japanese documentary classics and well-known economic and labor relations histories, vividly portrays the conditions of female workers in silk filatures and cotton-spinning mills from the early 1870s to the 1910s.

Tsurumi's approach is through the words of the factory girls and women documented in these works. Her subjects tell tragic stories of their lives and those of their co-workers. Seduced by the romanticized picture of modern factories painted by professional recruiters and urged on by a simple notion of filial piety, these girls and women in reality entered into indentured service. Their prizes were three meals a day and contract money paid to their destitute parents. As a way out of the harsh thirteen to seventeen-hour workdays, many committed suicide and many more became prostitutes. If they did not escape on their own, they died in fires in dormitories locked from outside or had their health destroyed beyond repair by tuberculosis in a matter of half a year.

Tsurumi's message is clear: some of the best-known commentators and historians of Meiji labor relations did gross injustice to these women by glossing over the stark reality of factory life. Tsurumi rebukes

Kuwada Kumazō, the prewar economist, who commented that for these girls "factory labor is only temporary employment. . . . Moreover, since they do it while in the lusty vigor of their youth, they endure what by comparison are extremely long working hours" (p. 172). She chastises Yamamuro Gunpei, the prewar Christian social activist, for failing to distinguish between traditional, nonindustrial work, which "permitted changes in tasks and routines," and factory work "performed under . . . rigid assembly-line conditions" (p. 141).

Perhaps because she lets her subjects speak of their conditions, Tsurumi overlooks opportunities to elucidate the rich historical backgrounds of these workers' thought and behavior as well as the customs surrounding them. For example, the employees of a silk-reeling factory in Yamanashi Prefecture staged a successful work stoppage in 1886. Tsurumi treats this affair merely as a spontaneous outburst of female workers with a healthy sense of fairness, which the company violated by practices such as holding back wages and calling it savings. The strike occurred at least ten years before the Western techniques of organized labor movement and collective bargaining were introduced into Japan. Where did these women get the idea of a strike? A clue is in their tactics, such as assembling on the premises of a temple in town to discuss and plan action. Gathering on temple grounds was a standard procedure when peasants rebelled in the Tokugawa period. It is worth exploring whether or not some arcane means of communication used by rioting peasants of the pre-Meiji period was passed to modern factory hands. Tsurumi misses another opportunity when she remarks that people "spoke of poor farmers 'selling their daughters to the mill or the brothel,' as if the two were the same fate" (p. 187). Not only were the two the same fate but also the Meiji method of selling daughters to the mill was identical to the pre-Meiji method of selling daughters to the brothel.

In the end, by pointing to today's trafficking of prostitutes from East Asia and Southeast Asia to Japan, Tsurumi seems to argue that the Meiji use of girls and women as disposable reflects the tenacious Japanese culture that dehumanizes women. One is reminded of the Confucian teaching inculcated in pre-Meiji Japanese that when a son is born, wrap him in fine silk, put him in a cradle, and give him beautiful toys, but when a daughter is born, wrap her in rags, put her on a mud floor, and give her a piece of clay to play with. It is no small irony that Tsurumi did not acknowledge the assistance that Hosoi Wakizō received from his wife, who was one such worker, in writing *Jokō aishi* (*The Pitiful History of Female Factory Workers* [1925]).

According to a Japanese saying, female silk reelers built the prewar Japanese navy. Although Tsurumi convincingly shows that "Reel for the glory of your Honorable Country!" was an empty injunction for factory girls, it was their labor that provided the Meiji

government with foreign exchange to buy arms from abroad. Their sweat and blood might have been put to better use.

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ANDREW GORDON. *Labor and Imperial Democracy in Prewar Japan*. Berkeley and Los Angeles: University of California Press. 1991. Pp. xvi, 364. \$34.95.

This is an ambitious attempt to revise our view of the political history of prewar Japan. Andrew Gordon takes the position that working people were more important, and more vocal, than we might have thought. He starts from a premise he ably defends, that the movers of twentieth-century Japanese politics directed their efforts at the masses, and labor was central to mass politics; appropriately, therefore, he uses the history of the labor movement to provide the data base for the book. If Gordon is right, we will have to rethink the trajectory of prewar politics.

The book is structured around several case studies of the political activism of workers. Gordon argues that three stages marked Japanese political history from the close of the nineteenth century to World War II. The first stage, "imperial bureaucracy" in the late Meiji period, gave way during the interwar years to "imperial democracy," which cultivated the masses and even drew its *raison d'être* from them. This second stage passed through both a movement and a regime phase, the latter coming after World War I. Progress toward true democracy in Japan was strong through the 1920s but collapsed in the face of overseas crises in the 1930s, culminating during early Shōwa in a new stage, the third, which Gordon chooses to call "imperial fascism." In his hands prewar Japan manifests genuine involvement on the part of the laboring masses. The basis for Japan's postwar democracy, on this reading, was laid early in the century when the movement for imperial democracy came to be the hallmark of Japanese politics.

This portrait of modern mass politics rests on a wide documentary base of materials from the labor movement, drawn notably from the Nankatsu region on the east side of Tokyo. Strike literature, campaign speeches and party platforms, and personal letters and statements give us vivid insights into the lives of workers who risked all for their belief in the virtue of political participation. To gather data Gordon visited archives and repositories of working-class activity, including the Ōhara Shakai Mondai Kenkyūjo. Well-chosen photographs add to the verisimilitude of the book's presentation.

Readers are likely to be fascinated by the author's case studies, even if they question some of his arguments. They may be troubled, for example, by the asymmetry involved in labeling the early stage of Japan's recent political history "imperial bureaucracy" (every regime has a bureaucracy) while the sub-

sequent two stages carry social-behavioral labels ("imperial democracy" and "imperial fascism"). There is also something problematic about a characterization that asserts the active engendering of history by forces from below, while portraying Japan as a society whose powerful elites manifest "a comparatively low threshold for social crisis" (p. 339) and are likely to overwhelm initiatives from below.

Gordon's study breaks new ground as it presents workers pursuing political goals. He could therefore do without quarreling over political labels and ideological proclivities, but he tries to recuperate "fascism" as a covering generalization for use in studying recent Japanese history. In an article I argued against the explanatory efficacy of "fascism in Japan," contending that epistemological limitations necessitate either that we make a leap of faith or that we employ some sort of qualifier ("Japanese" or "oriental" or whatever) to clinch the case ("A New Look at the Problem of Japanese Fascism," *Comparative Studies in Society and History* 10 [July 1968]). Gordon applies "imperial" as just such a qualifier and repeats the same epistemological points I raised, drawing the opposite conclusion that fascism is apt, even helpful, as a generic label. Perhaps "militarist" would be better, since military requirements plainly predominated. We could take a lesson from the approach of Hata Ikuhiko, whose valuable book on early Shōwa politics is titled *Gun fuashizumu undō shi* (History of the military fascist movement [1962]). Yamaguchi Yasushi, whose book on fascism Gordon leans on, tends to follow Maruyama Masao, who homogenizes "Japanese fascism" in order to bring it into general congruence with European varieties.

Despite these areas of concern, Gordon has succeeded in giving us a strong reading of a long and eventful time. This study offers a record of the vigor of workers in setting their own agenda from the moment of rising nationalism in the Russo-Japanese War (1904–05) to the advent of a conservative reaffirmation three decades later. We see the crowd in action, and we have good reason to wonder about its oft-alleged "docility." The book makes a major contribution to the historiography of prewar Japan; whether Gordon's stage theory (from bureaucracy to democracy to fascism) ends up in general usage is less important than his impressive industry and ingenuity in presenting the case.

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LEONARD A. GORDON. *Brothers against the Raj: A Biography of Indian Nationalists Sarat and Subhas Chandra Bose*. New York: Columbia University Press. 1990. Pp. 807. Cloth \$65.00, paper \$25.00.

After almost thirty years of the most painstaking research in the libraries and archives of Europe and

Asia, and after 150 interviews, Leonard A. Gordon has published this long-awaited biography of Subhas Chandra Bose, which includes the story of his brother, Sarat, also a freedom fighter. I know of no other comparable work that offers such meticulously detailed information on twentieth-century Bengali politics and politicians, both regional and national, Hindu and Muslim. Gordon should certainly be commended for his herculean quest to document the life of Subhas Bose, stretching from his birth in 1897 to his alleged death in a plane crash in 1945. The result is a masterful blending of archival evidence, acute intuitive apprehension, and gossip, brilliantly organized chronologically in fourteen chapters and amazingly confined to well within 1,000 pages. Much can be written in praise of this biography; it is surely Gordon's magnum opus.

Yet there is a disturbing aspect about a book on India's most controversial nationalist that lacks any historiographical discussion of Subhas Bose's much-publicized authoritarian personality, militarism, and fascist proclivities. Knowing Gordon's customary sophistication and objectivity, I was surprised at the absence of a chapter that conceptualizes the problem of Indian nationalism both in its regional context and in terms of its charismatic leaders. Surely, after immersing himself all these years in the political history of modern India, Gordon can give us more than those tired observations on the noble influences of Narendranath Vivekananda and Sri Aurobindo on Bose, or the equally tired anecdotes of Bose and his contemporaries that often show Bose in a favorable light. His "conclusions" are not conclusions at all in any comparative sense, but simply a summation. After countless references to how the Boses of Bengal sacrificed themselves on the altar of Indian nationalism, Gordon never fully explains what nationalism means. Considering the proliferation of violent struggles between regional minorities and dominant cultures in India, Pakistan, and Sri Lanka since the British departed, earlier assumptions on nationalism must be reexamined.

Was Bose—who lived in Nazi Germany from 1941–43 and courted Hitler—a fascist? Gordon seems to be of two minds on Bose's alleged Axis collaboration. Sometimes he feels that Bose was so busy working on behalf of Indian freedom against the dastardly British that he was not aware of the atrocities being perpetrated by Hitler and the Nazis. Bose evidently was not aware, according to Gordon, that at this time Hitler had ordered the death of some 250,000 Indians residing in Europe and known as gypsies; they were being murdered in the same places and the same manner as the Jews. Bose reminds us here of Gandhari in the *Mahabharata*, who throughout the epic wears a blindfold after learning that her husband-to-be was blind. Gordon's other image is of a Bose "willing to work with the devil to free India" (p. 282). The Japanese of World War II with whom Bose collaborated after the Nazi interlude were no

better than the Nazis. As the war crimes trials determined, the "Bataan Death March," the "Rape of Nanking" and the "Great Manila Devastation and Killings" were not isolated incidents of Japanese savagery and brutality. The Japanese murdered possibly as many as 35,000,000 Chinese, whom they held to be the Jews of East Asia.

In historical perspective, the book's facts leave no doubt that Bose was as pathetic and tragic as the Bengal whose heritage and culture he could not escape. He was violently anti-British, but it was under British tutelage that Calcutta enjoyed its golden age and Bengal was, for the first time, the most pivotal and creative region of India. When the British shifted their capital from Calcutta to Delhi in 1911, Hindu Bengal began its decline. When Bose surrendered himself to Indian nationalism, Bengali Hindu power and influence was shifting to Hindus from elsewhere in India and to Muslim elites at home. His dealings with the Nazis were monstrously absurd, and who can deny that when his opportunity finally came to play soldier and liberate India he proved a dismal failure? When that happened, as Gordon himself is forced to admit, even the most sympathetic Japanese officers "saw Bose as a military incompetent as well as an unrealistic and stubborn man who saw only his own needs and problems" (p. 517).

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BHARATI RAY. *Hyderabad and British Paramourcy, 1858–1883*. New York: Oxford University Press. 1988. Pp. xviii, 232. \$19.95.

This book by Bharati Ray charts the relations between the British government, the government of India, and Hyderabad, the premier Indian state, between the Mutiny of 1857 and the death in 1883 of Sir Salar Jung, dewan of Hyderabad for thirty years. Enemies were often given better treatment in the British empire than friends, who were supposed to find their own reward in being obliging. However hard Hyderabad tried to meet British expectations during this period, the British were always disappointed and usually distrustful. The book draws on Ian Copland's excellent study of relations with the princely states of western India (*The British Raj and the Indian Princes: Paramourcy in Western India, 1857–1930* [1982]) and Karen Leonard's earlier studies of Hyderabad ("The Hyderabad Political System and its Participants," *Journal of Asian Studies* 30 [1971]; and "Hyderabad: The Mulki–Non-Mulki Conflict," in *People, Princes and Paramount Power*, ed. Robin Jeffrey [1978]). But, owing to the range of its sources, especially its excellent use of private papers to plumb British motives and lay bare British rationalizations, Ray's book provides the most comprehensive study available. The usual caution should be given: apart from the papers

of Salar Jung himself, the bulk of the material used is British. Ray clearly exposes British hypocrisy and self-justification, but too easily takes Indians at their own word.

The first of three chronological chapters focuses on the demands made by the British on the Nizam to reward him for his loyalty during the Mutiny (Ray, unlike C. A. Bayly, argues that the Nizam, rather than the British resident, must be given credit for preventing the spread of unrest into south India and the Bombay and Madras armies (*Indian Society and the Making of the British Empire* [1988], p. 110). A second chapter considers the political implications of the abolition of the East India Company, which led the British to assume that former allies should accept the role of feudatories. This resulted in a prolonged argument about whether the Nizam need recognize British sovereignty or only British supremacy (Ray herself confuses the two [p. 81]). A third chapter details the attempts made by Salar Jung to prevent the British from using paramountcy as a vehicle for exploitation.

These three chapters are followed by two detailed case studies, overlapping the other chapters chronologically. One explains the efforts of Hyderabad to recover control of Berar (taken over by the British, supposedly temporarily, in 1853). The British explanation for keeping Berar, because of the value to Lancashire of its raw cotton, was questionable, for British industrialists much preferred American cotton. The second case study treats the history of railway building in the state. As the opportunities for economic development perceived by the Nizam's administration were superseded by the Indian Army's strategic calculations, the railway was diverted from the capital toward the British cantonment at Secunderabad. The Nizam was obliged to pay for it, however, and on increasingly burdensome terms.

At the center of the book lie two typical imperial formation dances. Both Salar Jung and the government of India tried to make use of the other. Although Salar Jung sometimes appears to be the classic imperial collaborator and turned to the government of India for support against both his rivals and the Nizam, he also hoped to turn the British against themselves by modernizing the administration of the state on a British model. When his plans aroused the suspicions of the viceroy, Lord Lytton, with his late-Romantic taste for backwardness and fear of the military prowess of Islam, Salar Jung tried to drum up support in London. He failed, of course; in a sad irony, he died just as the one viceroy who might have dealt with him fairly, Lord Ripon, expressed his willingness to reopen the question of Berar.

The maneuvers over Berar were the second of the prolonged dances performed by Hyderabad and the British. The territory was treated by Salar Jung as an integral part of the state, valued for its potential wealth, and he assumed that its return would be the obvious reward for good behavior during the Mutiny.

Instead, Berar aggravated the dispute over the railways. When pressed to contribute to their cost, the government of India offered a contribution from the revenues of Berar. As the British were supposed to hand over any surplus revenue to the Nizam, they were in fact taking away with one hand what they offered with another.

The book is a delight, a fine example of classical imperial history. The deceptions and disappointments portrayed are not new, but have rarely been so touchingly illustrated. Ray has done well by Salar Jung, and her account of Hyderabad should be read by everybody with an interest in the British empire in its heyday.

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GAURI VISWANATHAN. *Masks of Conquest: Literary Study and British Rule in India*. New York: Columbia University Press. 1989. Pp. x, 206. \$32.50.

This is a clever and elegantly clad little volume. Yet, for all of its apparent scholarship and sophistication, its central argument is simplistic, specious, and stale. Gauri Viswanathan argues that the English language was imposed as a device of deliberate deception, domination, and exploitation. The introduction of English into India was, as the dust jacket states, "a most effective form of political control," a device that "aimed at creating a sense of distance between the native Indian and his own tradition, further allowing the British government to strengthen its hold over its colonial subjects": "[t]he British goal was to fortify its own political position by means of instruction in English literature, through which Indian youth were to be persuaded of England's moral and cultural virtues." "[A] vital, active instrument of Western hegemony in concert with commercial expansionism and military action" (p. 167), "the English literary canon" was "put in the service of British imperialism" in order to hold the people of India in thrall. This "enabl[ed] the humanistic ideals of enlightenment to coexist with and indeed even support education for social and political control" (p. 3). Indeed, "the history of education in British India shows that certain humanistic functions traditionally associated with literature . . . were considered essential to the processes of sociopolitical control" (p. 3). No "easy solutions," such as dropping Shakespeare and Milton, should allow us to "cease to regard the uses to which literary works were put . . . as extraneous to the way these texts are to be read" (dust jacket, p. 169). And so, page after page, the tiresome cant and dogma of this currently fashionable and "politically correct" *puja* goes on and on—and on.

With astounding certainty, bereft of critical scrutiny, the author's eyes seem closed to anything but a monolithic motivation (with an implied collective guilt). Viswanathan's position is that knowledge is

power—without meaning apart from relations of power by which it is determined. Awkward facts or contexts are not allowed to stand in her way. A massively malevolent and hegemonic “Eurocentrism” shaped the imaginations and intellects and productions of writers and artists of the modern world. The possibility that Viswanathan’s own discourse may itself be a monocultural or Eurocentric imposition, precisely the same kind of “Orientalism” against which Edward Said inveighed, seems to elude her. The irony that, belying the author’s own Sanskritic *vamsa* (heritage), she might be relying almost entirely on evidence and arguments with which the British themselves attacked or debated each other seems to completely escape her. Viswanathan’s literary *nataraj*, dancing on an inert and prostrate India, is wholly British.

Overlooked are indigenous elements undergirding the rise of English in India. Formal English education in India actually began at least thirty years earlier than the author seems to realize; it began within the Hindu kingdom of Thanjavuru (Tanjore). Here English literature, taught to Maratha Brahman youths on demand by the German “Raja-Guru,” received a royal subsidy. Indeed, demand for English learning, both in arts and sciences, was so strong that thirty thousand Madras notables put their *cheveralu* (signatures) on a “public” plea for English-language establishments of higher learning and asked that funds from the Pachaiyappa Charitable Trust be applied to that end. By 1839, moreover, local scholars (native and European) had already devoted many years to perhaps the world’s first comprehensive projects for the description and preservation of the entire literary corpus of a civilization. (The rare manuscripts that they rescued are now kept in the Madras Oriental Manuscript Library.) Again, but for the lifelong energy and private expenditure of one local English official, Telugu language and literature might not have survived. Curiously, no other literary “masks” are allowed to explain, qualify, or compromise the colonizing character of English: no masks to cover the canons blended into the construction of modern Hinduism as a “public” religion; no masks to disguise the colonizing roles of Sanskrit, Persian, and other “classical” literary canons; no masks to hide the integrative conquests of a rising “Indish” (Anglo-Indian English) literary canon in its incorporation of indigenous canons; and no masks to conceal the strenuous support for vernacular education by official disciples of Sir Thomas Munro. Finally, no masks can cloak the vehement advocacy of vernacular languages and the fierce resistance to English made by missionaries who fought it precisely because it reinforced a continued dominance by the old Hindu gentry and ruling classes and, thereby, undermined their own radical social (and political, even anticolonial) agendas.

But human events defy such neat heuristic categorizations and glib historicism. Indeed, arguments linking literary study to British rule could just as well

be applied to the role of Brahman influence within the Raj. Would that human affairs were so simply explained, that interlocked causes and complexities were so easily reduced to dialectics of black and white, or that collective purposes could be so clearly discerned or implemented. The slippery proposition that, hidden for centuries untold, a single and faceless tradition was slyly imposed and that now, at last, this sinister ideology can be exposed beggars credulity. Such thinking may pass for scholarship or become high fashion in some academic circles. But the discipline and practice of history requires more. Some may be seduced or tantalized, albeit temporarily. And then this scholastic fad, too, will pass and be gone.

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DONALD HARMAN AKENSON. *Half the World from Home: Perspectives on the Irish in New Zealand, 1860–1950*. Wellington, New Zealand: Victoria University Press; distributed by Langdale, Gananoque, Ontario. 1990. Pp. 250. \$45.00.

This book stands in the tradition of Donald Harman Akenson’s previous work (for example, *Small Differences: Irish Catholics and Irish Protestants, 1815–1922*; Wellington, New Zealand: Victoria University Press; distributed by Langdale, Gananoque, Ontario. 1990. Pp. 250. \$45.00). which focused on the Irish, their migrations, and the role of religion in society. It is an impressive catalog of ethnic and demographic history, enriched by care and depth of scholarship and by facility of expression. The present volume is a history of a particular sort. Apart from the book’s discussion of the educational system, it only tangentially addresses Irish migrant contributions to the development of New Zealand’s national socioeconomic tapestry—its economic priorities, industrial relations, social welfare innovations, Pakeha-Maori relations, and issues of empire and national sovereignty.

Akenson is instead mostly concerned with who the Irish migrants were, how they happened to relocate, how they adjusted, and how they preserved or modified their religious, social, or national-heritage baggage. Much of the argument is refined from statistical data, and some from less-tangible but not necessarily less-plausible evidence. On the latter score, Akenson devotes a chapter to the career and writings of Dan Davin, an Irish-Catholic historian domiciled in England for much of his adult life. Davin is treated not simply as a literary figure, but as an expositor of Irish New Zealand customs, yearnings, and predilections.

Akenson is keenly aware of Protestant and Catholic Irish distinctions. Protestants, while themselves not homogenous, shed most of their Ulster Orangeism and were melded into the wider New Zealand society. Being “Irish” did not come to amount to very much. Irish Catholics also underwent a weakening of old

country national loyalties. Moreover, they were territorially dispersed across New Zealand and across urban/rural lines, in rough proportion to the general population. Bountiful land, generally prosperous economic conditions, and an egalitarian ethos in a new society, among other factors, avoided the crystallization of a distinctive, alienated Irish-Catholic underclass. But, while not ghettoized or oppressed, Irish Catholics sustained a special, even defensive cohesion through their Catholicism. It was decidedly Irish Cullenite, and it found its wellspring in the prolonged and burdensome efforts of the Catholic community to educate their children in Catholic schools and then to seek financial relief from the state.

There was a measure of public prejudice in New Zealand against Catholics, who until World War II were almost exclusively of Irish descent. This arguably was both cause and effect of the tenacity with which most Catholics clung to their faith and the religious educational structure that upheld it. This in turn fostered a horror of interfaith marriage and a suspicion of interfaith fraternization.

It is in such sociological reconstructions that the book's greatest utility lies and where Akenson's perceptions are sharpest. Irish contributions to the broader canvas of New Zealand political culture and socioeconomic enterprise are for another work, or for others to pursue.

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UNITED STATES

DAVID CHARLES SLOANE. *The Last Great Necessity: Cemeteries in American History*. Baltimore: Johns Hopkins University Press. 1991. Pp. xxiii, 293. \$35.95.

This book by David Charles Sloane joins a growing list of histories of cemeteries as correlatives of culture, landscapes, repositories of artifacts, and changing institutions. Antebellum rural cemeteries, like other institutions founded to build urban community, fulfilled diverse cultural functions rather than simply disposing of the dead (p. 69). They were a means for "renewing the past" and represented the conservative "interests, ideals, and philosophies of their promoters" (pp. 79–80); the poor were often left out by postmortem "privacy and exclusivity" (pp. 85–87). After 1862, national cemeteries had a "democratic atmosphere" and more simplified landscapes (p. 114). Turn-of-the-century cremation reform appealed to few. Commercialization transformed older cemeteries, and entrepreneurial speculation led to the professionalized funerary industry dominating today's "American way of death." Forest Lawn, the product of a suburban mentality, inspired modern "memorial parks" and anticipated Disneyland. Design and custom were sacrificed to business as new "cemeteryans" aggressively marketed services and products. During and after the 1930s large vertical corporations ap-

peared in some regions, controlling hundreds of cemeteries, funeral homes, and "memorial" dealerships. Mass production and the profit motive as well as changing tastes contributed to the evolution in cemeteries' appearance, distinctly different from those of other countries. Yet, because of this rich, diverse history and the quantity of older burial places that remain, often still operating, it is problematic to generalize about "the American cemetery," as Sloane often does: "The American cemetery is far from extinct" (p. 242). Indeed!

Sloane introduces Howard Daniels, Adolph Strauch, Ossian Simonds, Hubert Eaton, and others—influential cemetery theorists and designers. Strauch's "landscape lawn plan" made Cincinnati's Spring Grove one of the nation's most important landscapes, often imitated and even influencing the later public park movement; Strauch was as important an influence as Frederick Law Olmsted. Yet Sloane glosses over landscape aesthetics. Strauch and Olmsted did not have "similar views on cemetery design" (pp. 108–09), nor did Parsons "plan a Strauchian landscape" at New York's Pinelawn Cemetery (p. 140). Given the few illustrations, readers unfamiliar with different burial grounds and cemeteries learn little of landscape form. Sloane leaves the impression that most twentieth-century American burial places were efficient "lawn" cemeteries or "memorial parks"; he says nothing of the more typical rows of standardized, mass-produced stones crowded to bury the most people in the least space, maximizing profits.

Sloane's book falls short in tracing the transformation of American landscapes of death from the colonial era on. He suggests that private customs were inextricably tied to cultural forces such as the rise and decline of sentimentality, but he does not adequately probe ideas informing change, such as differences among Protestants. More could have been said about the impact of late-nineteenth-century sanitarians and developing technologies. Immigration increased funerary diversity, but the dimensions are bypassed in favor of mainstream cemetery typologies. Sloane's sweeping overview breaks no new scholarly ground. A chief weakness is generalization from secondary works, many factually flawed, and a failure to dig deeply in rich, complex primary documents. He focuses on New York State haphazardly. Living superintendents cited by Sloane, who might have produced valuable oral histories, were not fully and systematically tapped.

The book is flawed by many factual and interpretive errors, both trivial and major. For example, Sloane is wrong that "Puritans were more likely to follow English custom and bury their dead around . . . the meeting house" (p. 15), and that "the association of meeting house (or church) and graveyard was one of the distinguishing features of New England life" (p. 45). Calvinists deliberately secularized burials, disassociating them from the church. These were not places "of quiet meditation for the commu-

nity" (p. 22), but unsightly wastelands, even in rural towns, until antebellum aesthetic reform. New Haven's New Burying Ground drew no aesthetic inspiration from eighteenth-century English gardens (p. 32), and it was not "almost entirely relandscaped" (p. 42) in the 1830s.

It is wrong to assert that Mount Auburn (1831) and many other rural cemeteries were "designed by individuals without any engineering, horticultural, or design training" (p. 62). Mount Auburn was a non-profit corporation, not a "joint-stock company" that suggested "the concept of a private, for-profit burial place" (p. 59); and, despite Sloane's claims, the legal status of the cemetery was quite clear (p. 66). Philadelphia's Laurel Hill was a joint-stock enterprise, belying Sloane's assertion: "Although Americans refused to allow rural cemeteries to become commercial enterprises, they inevitably became symbols of economic success" (p. 59). Sloane even contradicts himself, noting examples of cemeteries with severe financial difficulties. Improbably, incorrect statements abound: "Slaves were particularly susceptible to losing their rights to burial places" (p. 15). Slaves had no rights, in life or in death.

Another major problem is stylistic. More rigorous editing might have eliminated repetitiveness and awkwardness. Arbitrary, small subdivisions and a weak sense of chronology produce confusion. Sloane's use of the passive voice masks an imprecise discussion of who did what and when. Sloane is optimistic and promotional, like contemporary cemeterians, lacking a historian's methodology or perspective. Readers unfamiliar with American cemeteries will come away with many misconceptions.

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ROBERT M. BLISS. *Revolution and Empire: English Politics and the American Colonies in the Seventeenth Century*. (Studies in Imperialism.) New York: Manchester University Press; distributed by St. Martin's. 1990. Pp. xi, 300. \$79.95.

Robert M. Bliss argues that politics in the American colonies, from their first settlement to the end of the seventeenth century, were determined in England. How colonial leaders acted in any given period depended on events in the mother country. The author surveys the entire empire, from Barbados to Massachusetts, for examples to support his contention.

Before the English Civil War, colonial elites used the charters and commissions of Charles I as their legal authority to govern. Colonial floundering during England's Interregnum reflected the mother country's own search for direction through the civil war, the death of Charles I, and Oliver Cromwell's attempts to find a legitimate government. Ironically, Massachusetts used its royal charter to take the place of the crown as a legal prop for its governing all of

New England during this period. After the restoration, relationships changed again. Edward Hyde, first Earl of Clarendon, returned to the policy of charters—corporate and proprietary—although colonial commerce would now figure in the new program of supporting the monarchy's ordinary needs by taxes on trade, channeled by the Navigation Acts. Thus, the new colonies of Carolina, New York, and the Bahamas—Jamaica an exception because of its peculiar circumstances—became proprietary instead of royal. After Clarendon's fall came yet another change, with Parliament giving extraordinary powers to colonial customs officers, hoping to gain increased revenue by enforcing trade regulations. The government also tried to gather as much information as possible about its empire. "Danby's government," the author argues, "had not set out to remake the empire, rather to make the empire work" (p. 189).

Bliss examines the views of English politicians toward the empire from 1660 to 1683. He argues against Stephen Saunders Webb's view that the government, as a matter of militaristic policy, sent more and more soldiers to the New World as governors (*The Governors-General* [1979]). He points out that in several instances military experience did not appear to be the major reason for appointments, and that in any event, given England's history, governmental structure, and the social class from which governors came, most of them would have had military experience of one sort or another. He examines the different views of the Duke of York and the Earl of Shaftsbury about the nature of colonies and how they implemented those views, giving considerable attention to the remarkable and unworkable Fundamental Constitutions devised by Shaftsbury (with John Locke's help).

Bliss concludes by relating events of the 1680s, when in reaction to past policies the English government took away the autonomy of some colonial governments and replaced others with new mechanisms, actions that mirrored the remodeling of borough charters and franchises in England. Colonies, too, reflected England's revolutionary settlement of 1689. More people participated in government, while colonies themselves changed from branches of English local government into "fully-fledged polities, parallel with England if still subordinate to it" (p. 244).

Throughout his work Bliss questions many interpretations of English-colonial relations. His book is based not so much on new sources unearthed as on reinterpreting those already published, although he has used to good purpose material available in the British and Bodleian libraries. It is an interesting survey and a solid contribution to our knowledge of the seventeenth-century English empire in America.

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DAVID S. SHIELDS. *Oracles of Empire: Poetry, Politics, and Commerce in British America, 1690–1750*. Chicago: University of Chicago Press. 1990. Pp. xiv, 295. \$29.95.

For decades the study of literature in early America has centered on Puritanism. More recently scholars have explored Puritanism's connections to revolution and nation building. Searching for the textual roots of the American Renaissance and of modern American literature, much of this work begins with assumptions about the continuity of an American exceptionalism that most historians find dubious at best. David S. Shields's early American literati are neither Puritans nor revolutionaries, nor does their writing obviously anticipate any subsequent school of American literature. Nonetheless, Shields paints a picture that will be familiar to historians, if not to literary scholars. His Georgian writers and scribblers are not yet Americans but still very much British colonials whose writing was powerfully shaped by the structures and circumstances of eighteenth-century empire.

Shields is an "historicist," although perhaps more "old" than "new." His central concern is to situate the scattered, ephemeral poetry and belles-lettres regarding public affairs in the eighteenth-century American provinces firmly in the context of their production. His discussion moves through time, shaped by broad themes of mercantilism, commerce, and agriculture; the politics of governors and assemblies; and imperial warfare. Throughout, Shields grounds his understanding of his reading of a diverse poetry in a solid account of the political economy, politics, and diplomacy of empire. The first two chapters discuss the Georgian poetry of empire, shaped by a heroic vision of peaceable British commerce, triumphantly grounded in the new stability of British institutions and its victory in the wars ending in 1713. "Material redeemers" such as William Penn and James Oglethorpe provide a way to celebrate the empire as benevolent and progressive. Chapter 4 charts a transformation of writing about plantation production from a simple extension of commerce to a transforming, creative enterprise, "heroic agriculture." Part 2 moves on to the politics of the "prerogative" in Massachusetts and New York, in which "literary performance" was put to service in the struggles over the governor's salary and the authority of the assembly. Part 3 explores Anglo-American poetic rhetoric defining the external others, the Spanish and the French empires, and that rhetoric's role in shaping and articulating the xenophobic animosities contributing to the wars that followed Robert Walpole's peaceable commercial empire.

One of the central virtues of Shields's study is his concerted attention to the poetry of the Georgian empire as "works of political art" (p. 228). Shields develops a fascinating picture of the way in which ballads and poems acted as rhetorical instruments in the pre-party politics of the eighteenth-century colonies. His discussions of the "power of fable" and the

"ballad wars" in the Morris-Cosby controversies (pp. 149–69) are a wonderful contribution to our emerging understanding of the ways in which a "public sphere" of clubs, taverns, and popular culture provided the forum for political struggle and consensus in both Britain and the colonies. In this regard Shields makes his most important methodological departure from the norms of his field, insisting that the published "canon" is insufficient for understanding a process of literary production and consumption in which much of the material circulated in manuscript form in a club culture. Shields introduces a wealth of political poetry from relatively untouched manuscript sources and pays close attention to previously neglected work published in newspapers. A detailed regional bibliography provides access to this material.

By his own admission, however, Shields can only scratch the surface. The imperial corpus he examines has been ignored by literary scholars in search of a more useable past, so he must lay out an interpretive groundwork that may strike the historian as obvious: poetry commenting on public affairs was deeply rooted in those very same public affairs. Thus, while it is full of useful insights and fascinating new texts, this study falls short of its potential utility for historians as a study of the play of rhetoric on an audience. Throughout the book Shields describes the circumstances shaping the writing of a piece, but he stops short of exploring the circumstances shaping its reception by a specific reading public. He is very much aware of his "circumscribed field of inquiry" and promises to present the "reception history" in a "subsequent study" (p. 9). But in his conclusion Shields also poses some important questions about the transition between his imperial poetry of commerce and the republican poetry of agrarianism that supplanted it with the revolution. Historians will benefit whichever direction he chooses to move from this solid, serviceable, and elegantly written book.

JOHN L. BROOKE
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LARRY GRAGG. *A Quest for Security: The Life of Samuel Parris, 1653–1720*. (Contributions in American History, number 142.) New York: Greenwood. 1990. Pp. xix, 214. \$39.95.

Samuel Parris is remembered for his role in a single brief episode, the Salem witchcraft crisis of 1692. Now, 300 years after that event, Larry Gragg has published the first full-scale biography of this man.

Apart from the Salem Village years, the archival record on Parris is sketchy, but Gragg's exhaustive research offers as full a story as we are likely to get. He recounts the careers of Samuel's father and uncle, who were merchants and Barbados sugar planters; traces his brief Harvard attendance in the early 1670s and stint as a Barbados sugar agent and plantation

owner after his father's death; his mercantile efforts in Boston; the Salem years; and his final years as a frontier minister, school master, and petty merchant in Sudbury.

Gragg challenges in some respects the treatment of Parris in Stephen Nissenbaum's and my book *Salem Possessed: The Social Origins of Witchcraft* (1974), which emphasized Parris's early economic frustrations and suggested that this helped shape his response to the turmoil in Salem Village. Gragg, by contrast, argues that the 170-acre Barbados plantation Parris inherited from his father "must have provided . . . a comfortable living" (p. 13) and that he enjoyed "modest success" as a Boston merchant.

Yet he concedes that no evidence exists on what income, if any, the Barbados plantation produced, or what price it fetched when Parris left the island. In view of the drastic decline in sugar prices in these years, not to mention the catastrophic hurricane of 1675, the inferential case for viewing the Barbados years as at best anxiety-ridden seems strong. His Boston merchant years left scarcely a trace in the records. The most compelling piece of evidence perhaps is the obvious one: Parris's decision in 1688 to abandon his fifteen-year quest for commercial success and enter the ministry.

On Salem witchcraft, Gragg adopts a multicausal approach, mentioning poor harvests and Indian war along with the conflict between traditionalistic and market-oriented villagers. He emphasizes, too, the reality of witchcraft fear, which he believes modern interpreters tend to underplay. Gragg notes Parris's efforts in 1692 to read the best available treatises on witchcraft, such as William Perkins's *Discourse of the Damned Art of Witchcraft* (1608) and Richard Baxter's *The Certainty of the World of Spirits* (1691), but stresses Parris's fundamental mistake in giving the afflicted girls a forum rather than isolating them—the course advised by Cotton Mather.

Drawing on his careful reading of Parris's sermon book and of the published records of Salem Village and its church, Gragg recounts the sequence of events clearly and insightfully. He is particularly good on the post-1692 period, when the anti-Parris faction, thwarted at the local level, turned to outside authorities to achieve their purposes.

Parris deserves to be remembered, Gragg argues, not solely for his role in the events of 1692 or his exceptionalism but for his very ordinariness as a Puritan minister, affectionate husband and father, and man of his time normally concerned with material security for his family. Certainly the recurring warm allusions to kissing in his sermons and the image of the elderly paterfamilias buying kitchenware for his about-to-be-married daughter help counter the temptation to turn him into either a monster or an idealized abstraction of thwarted ambition at the dawn of the capitalist era. This fine biography rewards the reader with many perceptive

insights, but Parris will probably continue to fascinate successive generations not as a Puritan everyman but for his activities during those tortured months of 1692.

PAUL BOYER
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MERTON L. DILLON. *Slavery Attacked: Southern Slaves and Their Allies, 1619–1865*. Baton Rouge: Louisiana State University Press. 1990. Pp. 300. Cloth \$39.95, paper \$16.95.

Merton L. Dillon contends that from the day the first slave was introduced into the American colonies black bondage was doomed. Casting a wide net, he proposes that the enemies of slavery were the allies of the slave: sometimes Native Americans, sometimes non-slaveholding whites, sometimes Enlightenment-style humanitarians, sometimes northern abolitionists. The allies of slaves were not only individuals or groups but they were also foreign powers such as Spain, France, and England, which over two centuries threatened slavery by threatening the dominant white culture and provided runaways aid and comfort (as did the British during the American Revolution or the Spanish in Florida). Other allies included domestic politics and the economy: Free Soil, Liberty, or Republican parties that were, southerners claimed, tools of abolitionists; a capitalist entrepreneurialism, ill-fitted to the South, that called the plantation economy stagnant; and a labor system that putatively announced impending crisis.

Describing these varied slave allies—call them simply threats to the peculiar institution—Dillon presents a complexity of issues, individuals, institutions, attitudes, and ideas that ultimately bred the Civil War and destroyed slavery. This is a familiar story that he tells well, and he develops the theme of allies as perhaps none has quite done before. That is laudable, for Dillon is undoubtedly the dean of antislavery historians in America today, a thorough scholar well versed in both the traditional and recent literature of his field.

But Dillon is preeminently a narrative historian, a solid storyteller, as his *The Abolitionists* (1974), *Elijah P. Lovejoy* (1961), and *Benjamin Lundy* (1966) splendidly attest. He is not comfortable with involved analysis. That is too bad, for we are deprived of much that might have been probed. Take the example of the law. We read fleetingly about specifics such as personal liberty laws, the Emancipation Proclamation, the orders of generals John C. Frémont and David Hunter, or the uneven enforcement of slave codes. But we learn nothing, for example, of aid to slaves in or out of southern courtrooms or of tactics North and South to skirt oppressive law by legal argument and practice. Given his imaginatively wide-ranging inclusion of allies, Dillon might have done more with

examining law as an ally. Furthermore, read simply as a story this book is perplexing. Commendably, Dillon balances each gain with its opposite loss: fears of slave revolt and the failure of its fulfillment; Virginia's constitutional debate over slavery and the subsequent scuttling of emancipation. There is nothing inevitable in a story thus told. Yet there is a pervading sense of inevitability throughout the book. Without doubt the slaves will be free. The toggling of his evidence, however, hardly supports such determinism.

At the outset Dillon tells us that his book "is about the slaveholders' long travail and their ultimate failure . . . the changing relationship of slaves to other groups within the South and beyond it . . . [and] the ways in which slave resistance was linked to foreign relations, war and invasion, and to internal social and political conflict" (p. 3). That promises much and the book tells an intriguing story. Analytically, it succeeds less well.

WILLIAM H. PEASE
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NORRECE T. JONES, JR. *Born a Child of Freedom, Yet a Slave: Mechanisms of Control and Strategies of Resistance in Antebellum South Carolina*. Hanover, N.H.: University Press of New England, for Wesleyan University Press. 1990. Pp. viii, 331. \$35.00.

Norrece T. Jones, Jr., has written a passionate book that restores the element of tragedy (if ever it has been absent) to American slavery. While many recent works have stressed autonomy and creativity within the slave community—propositions that Jones would not reject—he believes that they neglect the "context" within which the community developed, an oversight he intends to correct. His book is close to an old-fashioned "whips-and-chains" story about slavery, a characterization that fails to do him justice, however, for it is much more sophisticated. Indeed, he stresses the separation of families as the single most cruel and effective punishment and control possessed by the planter class. Discounting the paternalistic compromise as uncharacteristic, he views slavery as a state of perpetual warfare between slave and master, with one trying constantly to outwit or otherwise overcome the other. He considers everything from work and family structure to individual activities and social organization among slaves as part of a wide-ranging and interlocking mechanism for slave manipulation. Using folk tales, aphorisms, songs, reminiscences, and narratives, he admirably suggests how the slave's perspective on religion, family life, and relationship with (to use Charles Joyner's telling phrase) "those who claimed to own them" diverged from that of the claimants, and he considers how the laborer's values and outlook contributed to the development of a separate and largely closed slave community. Closed, that is, until the master intervened through Christianity, drivers, snitches, and other insidious means.

Jones views every point of contact between black community and white, not entirely without reason, as an avenue of planter control. But, it seems to me, his conceptualization gives the master too much power and the servant too little. Indeed, for one who concedes vitality to the slave community, he gives it surprisingly little responsibility for or influence in shaping events. Perhaps that would smack too much of a compromise that he rejects. Yet failure to take that influence into proper consideration—to acknowledge the give-and-take involved in slavery—hampers his judgment and his story.

Two examples suffice. First, it is simplistic to argue that garden plots and task labor were implemented by the ruling class in part "to instill in slaves an avid individualism that was intended to pave the way for collaboration" (p. 104). Scholars have differed about development of the task system and whether it was imposed from above or forced from below, but James H. Hammond, for one, sought to discourage it. In that intention he was opposed, ultimately with success, by his servants. Second, while it may be true that the interaction of blacks and whites in Christian congregations served to sap the independence and dilute the revolutionary potential of the slave community, the dynamism of interchange and the opportunity for exploring mutual influence is missed when emphasis is preeminently on the white use of black people. The chapter on religion is perhaps the most interesting in the book but it promises more than it delivers.

Finally, Jones has not been well served by his publisher, for the text abounds with infelicities and ambiguities that a firm editor could have eliminated. He uses such phrases as "divine guise" (p. 140), "antiservile characteristics" (pp. 53, 105, among other places), "multiracial freemen" (p. 130), and "religious coffers" (p. 143) in ways that are idiosyncratic at best, possibly incorrect, and jarring if not confusing. Moreover, he sometimes presents evidence in such a way that one has to strain to gain his meaning (see, for example, pp. 61, 142, 144, 147, 159). When one does ascertain meaning, it is not always clear how the evidence supports the case presented; alternative interpretations exist. He is occasionally too glib. Although Hammond has received much notoriety in recent years, John C. Calhoun for one might be surprised to hear Hammond described as "perhaps the most famous defender of the South" (p. 202). These lapses are all the more regrettable because where Jones is properly focused and restrained, his book reads quite well. There is much of value here. If one can suppress irritation at the flaws that prevent the book from being as good as it could be, it is worth reading.

DANIEL C. LITTLEFIELD
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PETER CHARLES HOFFER. *The Law's Conscience: Equitable Constitutionalism in America*. (The Thornton H. Brooks Series in American Law and Society.) Chapel Hill: University of North Carolina Press. 1990. Pp. xiv, 301. Cloth \$32.50, paper \$12.95.

Equity emerged in medieval England as the body of rules and procedures applied by the chancellor's court to provide remedies when the law courts offered inadequate relief. As commonly told, the history of equity in American jurisprudence is the story of the gradual merger of equity courts and law courts and of procedures for actions in equity and at law. Peter Charles Hoffer gives equity a far more prominent position than it is usually given in American legal history. Hoffer argues that the equitable themes of "trusteeship," "equality," and "reality," which developed in disputes between private litigants in equity courts, have also made an important contribution to American constitutional law.

Hoffer traces this "equitable constitutionalism" from its English origins to the present day. For Hoffer, *Brown v. Board of Education*, the famous United States Supreme Court school desegregation decision of 1954, is the most important equity case in American legal history. In a unique reading of the case, Hoffer describes the justices in *Brown* as chancellors in equity who saw it their duty to provide a just remedy for the legacy of Jim Crow. The *Brown* decision may have been with uncertain precedent in American constitutional law, but it was consistent with the equitable traditions of trusteeship, equality, and reality. As exemplified by the mixed progress in achieving school desegregation after *Brown*, Hoffer emphasizes that there has often been a gap between the ideals of equitable constitutionalism and the actual performance of the courts in racial equality and other areas of constitutional law.

This book makes an important contribution to the literature on the history of equity in American jurisprudence and presents a thesis that will provoke significant discussion. In its narrow sense as a history of equity courts and equitable doctrines, the book is an impressive work of scholarship that is at once detailed in its treatment of numerous subjects and still highly readable. The book is an ambitious study of equitable constitutionalism, and its arguments are boldly stated with flair and style. Particularly strong points include the discussion of Thomas Jefferson's Declaration of Independence as a pleading for equitable relief, the treatment of jurists in the early twentieth century such as Oliver Wendell Holmes and Roscoe Pound, and Hoffer's epilogue on the affirmative action decisions of the 1970s and 1980s.

The debate on this book will center on whether Hoffer has given too prominent a place to the influence of equity in constitutional law. Trusteeship, equality, and reality may well be important themes in American constitutional history, but are these themes

the heritage of the equity courts or are they more properly viewed as products of the common values that have informed both equitable remedies and the Constitution? Furthermore, Hoffer has provided what is largely a doctrinal history of equity and what he has identified as equitable constitutionalism. Within the scope of this doctrinal history, Hoffer is able to show, almost too serendipitously at times, the similarities between certain language and themes in both the equity courts and in constitutional jurisprudence. It remains to be shown in a broader social and political sense whether the contribution of equity to our legal culture has been as great as Hoffer claims.

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DAVID P. CURRIE. *The Constitution in the Supreme Court: The Second Century, 1888–1986*. Chicago: University of Chicago Press. 1990. Pp. xiv, 668. \$70.00.

This is the second and final volume of David P. Currie's monumental study of the United States Supreme Court. In the first volume, Currie traced the Court's history from its founding in 1789 to the death of Chief Justice Morrison R. Waite in 1888. The second volume begins with Chief Justice Melville W. Fuller in 1888 and closes with the retirement of Chief Justice Warren Burger in 1986. Each of this book's eight "parts" is organized around the term of each of the Court's eight modern chief justices. Currie further divides the narrative into chapters that cover such diverse topics as the protection of economic interests, the New Deal, privacy and equality, and separation of powers.

The work is something of a tour de force when taken on its own terms. As Currie explains: "my aim is not simply to tell the story of how the Justices interpreted the Constitution but also to analyze and criticize their work from a lawyer's point of view" (p. xiii). Currie succeeds admirably in mobilizing his traditional organizational scheme behind this objective. He cogently explains the doctrinal shifts that accompanied the Court's transition from a docket filled with economic due process and contract and commerce clause issues in the late nineteenth century to a new agenda dominated by the due process revolution in civil liberties and civil rights during the last half of this century.

Currie casts a critical eye over the history of judicial activism—the justices' substitution of their values for those of the framers of the Constitution. Throughout, Currie questions the Court's use of its judicial authority, sharply criticizing its excesses in the era of economic due process and more recently in civil liberties and civil rights cases. Yet he balances his

skepticism of judicial review with a healthy respect for the justices' unavoidable task of maintaining separation of powers and limited government under the rule of law. Currie avoids the rhetorical excess that has dominated the work of scholars critical of judicial activism, such as Raoul Berger, while simultaneously making the point that, although the justices have sometimes exceeded the scope of judicial review, they have exercised that power with sufficient prudence to make the Court a pillar of the constitutional system. Currie concludes that "the record of the first two centuries suggests that most of the time the Justices have appreciated both the demands and the limits of their power" (p. 604).

It is important, however, to appreciate what this book is not. It is not a synthesis of existing scholarship, for Currie draws disproportionately from the legal as opposed to the historical literature; it is not a charming survey in the style of Robert G. McCloskey's *The American Supreme Court* (1960); it is not an exercise in the careful working of primary sources, for most of the copious footnotes cite published case reports instead of the justices' private papers, newspapers, and such; and it is not a history of the Court that seeks, except in a nebulous way, to connect it and the Constitution to the social, cultural, and political developments of the past century.

Currie has produced a valuable scholarly reading of the Court's internal doctrinal development and the justices' roles in formulating it. In the end, however, most constitutional historians, accustomed as they have become to an external perspective on the Court, are likely to feel as if they have watched America's greatest constitutional volcano erupt without much explanation of either the subterranean forces that prompted its blasts or the subsequent changes that its debris wrought on the surrounding landscape. Instead they are made the beneficiaries of exactly what Currie intended, a lawyer's legal history with a vengeance.

KERMIT L. HALL
University of Florida

JACOB RADER MARCUS. *United States Jewry, 1776–1985*. Volume 1. Detroit: Wayne State University Press. 1989. Pp. 820.

Jacob Rader Marcus is a legend in his own time. Scholar, teacher, and the founder and director for well-nigh half a century of the American Jewish Archives, he is the author of numerous works on American Jewish history. Now at the age of ninety-five and still going strong, he has projected yet another multivolume study that will trace the history of American Jewry from 1776 to the present.

This, the opening volume, begins where his classic, three-volume *The Colonial American Jew* (1970) left off. Dealing topically with the Jewish experience from 1776–1840, it provides a detailed synthetic history of

a hitherto neglected era. (*The Jews in the United States* [1963], edited by Joseph Blau and Salo Baron, is a collection of documents for a shorter period, 1790–1840.) The importance of those years for America's Jews cannot be minimized. At a time when Americans were writing and rewriting constitutions and were transforming a colonial-oriented economy into an expanding continental system, Jews were accorded greater political freedom and economic opportunity than ever before. Simultaneously, new demographic patterns, most notably the immigration of large numbers of German Jews, sparked radical changes within the Jewish community itself; the earlier synagogue community (*kahal*) disappeared, social and philanthropic institutions became secularized, and the modification of traditional religious practices accelerated.

Marcus skillfully describes the integration of the rapidly Americanizing Jews into the life of the young republic, exploring their successes and their failures, their leaders and even their criminals. Sketches of countless individuals, constructed from largely unused primary sources, highlight his rich account. The wealth of detail also validates one of the author's stated purposes: "I am . . . committed to the thesis that the story of the Jew in this land lies not in the vertical eminence of the few but in the horizontal spread of the many" (p. 16).

As one who posits the centrality of religion to Jewish existence, Marcus not surprisingly devotes roughly one-third of the book to religious behavior and to Jewish socioreligious institutions. From theology to synagogue budgets, from the role of the rabbi to the problems of educating Jewish youth, his analysis is extensive. Rounding out the portrait of a minority that strove to reconcile a desire for religious and ethnic identity with a seemingly antithetical pull to integrate fully in the host country, he considers the position of Jews, singly and as a group, in Christian America. His survey of interreligious encounters includes Christian missionary activities, Protestant influences on the synagogue service, intermarriage between Jews and Christians, as well as various manifestations of Judeophobia.

Written in his characteristic descriptive style, Marcus's study is a valuable contribution to American Jewish historiography. We eagerly await the next three volumes.

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JEROLD S. AUERBACH. *Rabbis and Lawyers: The Journey from Torah to Constitution*. Bloomington: Indiana University Press. 1990. Pp. xix, 249. \$29.95.

Jerold S. Auerbach, author of two fine studies on civil liberties and the experiences of American Jews with prominent legal firms in the first six decades of the twentieth century, has now embarked on a personal

odyssey trying to understand and reconcile the competing demands of Americanism and Judaism and the efforts of rabbis and lawyers to blend them into one. In a series of compelling and well-written essays, primarily biographical vignettes of prominent American Jews (such as Louis Marshall, Louis D. Brandeis, Felix Frankfurter, Stephen Wise, and Joseph Proskauer) and historical analyses of the Bible and the writings of the prophets, Auerbach provides a rationale for how American Jews were able to harmonize their Jewish traditions with the pull of American demands for acculturation.

Auerbach stresses how the Jewish leadership, mostly if not exclusively German-Jewish immigrants and their sons, provided a framework and an explanation for blending Torah with constitutional principles. The argument is certainly a novel one and is well worth pursuing, but it needs refinement. The men highlighted in this study did not come from East European Jewish backgrounds, as did an overwhelming majority of American twentieth-century Jews; they did not represent these people in any but the most peripheral ways and could not exert authority over them in any meaningful fashion. One wonders, therefore, how representative they were in establishing the terms of Americanization.

To be sure, some people of East European ancestry tried to blend their values into some kind of acculturative process, but the outstanding characteristic of American Jewish groups, especially those in the New York City area where a majority of the immigrants and their children lived, was the diversity of their interests and expressions. Not until the advent of Hitler and the annihilation of European Jews during World War II was there anything like unanimity within the American Jewish population on any subject. Then sixty-three of sixty-four major Jewish organizations took a stance on the establishment of a Jewish state in Palestine and perhaps 85 percent of America's Jewish population endorsed Zionist goals. When that happened, however, few of the German-Jewish elite were there to lead where their coreligionists wished to go. They were too enamored of their own positions and fearful of the increasingly precarious situation of Jews in the United States to publicly lead a charge against the Franklin D. Roosevelt administration.

Auerbach's analysis will undeniably provoke discussion among those concerned with how Jews acculturated and reconciled their Jewish heritage with their commitments as Americans. I suspect, however, that most American Jews do not think about that subject, are Jewish in whatever way pleases them, and are primarily concerned with getting by in their day-to-day lives without any philosophical analysis. As successive generations of Jews move farther away from their European heritage, they tend to lose much of what was once considered, without any intellectual rationale necessary among those of East European ancestry, their *Yiddishkeit*. They are Americans in

myriad ways and have less concern about how their lives as Jewish Americans have evolved than Auerbach supposes. Nonetheless, for individuals curious about one man's attempt at reconciliation of both Jewish and American values, this book is well worth examining.

LEONARD DINNERSTEIN
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THERON F. SCHLABACH. *Peace, Faith, Nation: Mennonites and Amish in Nineteenth-Century America*. (The Mennonite Experience in America, volume 2.) Scottsdale, Pa.: Herald. 1988. Pp. 415. \$19.95.

This book, the second in a projected four-volume series on "The Mennonite Experience in America," marks a major milestone in American Mennonite studies. Working with an impressive mastery of the wide-ranging sources, Theron F. Schlabach provides a needed corrective to older accounts that tended to view the nineteenth century as a time of stagnant and uncreative traditionalism. In Schlabach's story nineteenth-century Mennonites struggled creatively and vigorously with the powerful forces of modernization at work in American culture. The result was a steady interplay of accommodation and resistance, a dynamic that Schlabach chronicles with great subtlety.

Two early chapters focus on Mennonite social history, particularly on the pursuit of land and wealth and the nature of communal life. Like many Americans, Mennonites aggressively sought land and prosperity, but unlike many others their emphasis on humility set limits on acquisition. Although remaining fairly steadfast in pacifist convictions, they held few scruples about settling on lands made available through violence against Indians. In exploring the inner life of Mennonite communities, Schlabach treats the fear of frivolity, the ambiguities of pleasure, music, and attitudes toward sex and marriage.

Turning to Mennonite faith and practice, Schlabach focuses on the inexorable and often rending tensions between tradition and modernity. Both traditionalists and progressives invoked the theology of humility that remained dominant until about 1870. But progressives incorporated new elements—revivalistic views of conversion, American notions of progress and self-improvement, and denominational ecclesiology. The traditional view of politics as a realm apart from religion became somewhat blurred, particularly during the Civil War, and Mennonites became more open to politics as an instrument of divine purposes. In response traditionalists led "Old Order" schisms, reaffirming humility and the established order. Here especially Schlabach admirably combines empathy with critique. He insists that to understand the Old Order one must suspend certain modern assumptions—for example, that change means progress, that the individual reigns supreme, and that events in New York City are inherently more impor-

tant than those in Yellow Creek, Indiana. In this light he sees leaders who developed "an intelligent and coherent system" of ideas, a "brave vision of living in harmony with creation rather than always pursuing change" (p. 229).

After two chapters treating the large immigration of Russian Mennonites between 1873 and 1883, Schlachach closes the volume with an overview of "the quickening at century's end." Progressivism, he concludes, produced four basic changes: "a more outward-looking vision," more Protestant and American types of church structures, "separation of salvation from Christian practice," and an exchange of humility for aggressiveness (pp. 301–21).

With his even-handedness and critical rigor, Schlachach has moved far beyond traditional denominational history. His story abounds with failure and irony on all sides. Nevertheless, I wish he had worked as hard to understand why progressives charted new courses as he did to understand why traditionalists held the line.

C. LEONARD ALLEN
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VIRGINIA LIESON BRERETON. *Training God's Army: The American Bible School, 1880–1940*. Bloomington: Indiana University Press. 1990. Pp. xix, 212. \$27.50.

Conservative Christianity has received a good deal of scholarly attention over the past two decades. As a result, old assumptions have given way to a more balanced appraisal of the origin, content, and appeal of groups within that tradition. Virginia Lieson Brereton's book is a wonderfully written addition to the field and fits within the best of this trend.

Some will disagree with Brereton's choice of words in the narrative. Painting with a broad stroke, she includes Pentecostals in the family of Fundamentalists, and it is sometimes unclear what difference she intends between the term fundamentalist and the broader term evangelical. To her credit, she is honest about the dilemma. In a superb appendix, she acknowledges the difficulty that scholars have had in coming to a consensus on the issue of labels and admits that her definition of fundamentalism is a bit looser than that used by some other scholars.

Brereton's broad definition is conditioned by her topic. She investigates the uncharted territory of evangelical Bible schools, lending support to Ernest Sandeen's assertion that Fundamentalists survived the debacle of the 1920s by ingenious use of these educational institutions. Brereton writes passionately about the schools, placing them (some will find ironically) within the late-nineteenth-century movement toward practical, vocational education à la John Dewey. Noting that normal schools were, in effect, sister institutions to the Bible schools, she concludes that they "deserve at long last to take their places in

this catalog of practical and vocationally oriented institutions" (p. 156).

Brereton traces the late-nineteenth-century emergence of Bible schools to a desire to train lay workers, especially women and older men, for assignments in home and foreign missions. Coming out of the larger "training school" movement that originated in Europe a generation earlier, these schools initially sought to be "complements rather than . . . competitors to theological seminaries" (p. 67). With the Bible as the "chief textbook" and with teachers whose strength "lay in their religious dedication . . . not their academic credentials" (p. 70), the schools grew, expanding their programs in the early twentieth century. When seminaries began to offer similar practical courses, some of the "training schools" faded out; the Bible schools survived, however, because they came to play a crucial role in the Fundamentalist movement that emerged by the 1920s.

As the schools grew, they tended to become more structured, in part because their student bodies changed. Younger students required more rules, as did newcomers not specifically committed to a religious vocation. Schools also began to move toward academic respectability. During the years following World War II, the Bible schools established their own accrediting agency, the American Association of Bible Colleges, and some even sought and received the accreditation of the regional associations governing liberal arts colleges.

Brereton includes a helpful bibliographic essay, offering the reader valuable insight into the sources and debate that provide the framework for her study. Her book is a major contribution to the literature on fundamentalism and on American education. That it is delightful to read simply adds to that contribution.

JAMES R. GOFF, JR.
Appalachian State University

L. RAY GUNN. *The Decline of Authority: Public Economic Policy and Political Development in New York, 1800–1860*. Ithaca: Cornell University Press. 1988. Pp. xiii, 284. \$29.95.

Most American historians even as late as the 1940s framed their interpretations of political change in the early republic in terms of a manichean Hamiltonian-Jeffersonian conflict. In those terms the 1820s–1860 period was portrayed either as a chapter in the old-line "presidential synthesis" or else as a well-defined "reform era" in which antislavery was the culmination of converging movements. As L. Ray Gunn shows with brilliant force and clarity, a historian reappraising antebellum political change today is able to frame that period's issues and developments in a different "structural" context, drawing on the rich literature of studies since the 1960s in legal history, the "new" political history, and development theory.

Gunn's purpose in this fine work is to examine from these new perspectives the complex and often wonderfully mysterious history of New York government and politics in the pre-Civil War era. For better or worse, he has eschewed study in the collections of politicians' private papers that abound for that era, instead basing his analysis mainly on the public documents of the legislature, the administrative bodies, and the courts to illustrate "the nature of governance" (p. x).

Gunn's core argument is that in the early decades of the nineteenth century New York had a coherent political economy—that is, that the state government, with power strongly centralized in the legislature, extended the mantle of patronage to private enterprise in the form of privileges and immunities, outright subsidies, and other types of aid. The regulatory element was subordinated to a distributive style in which multiple interests received ramifying programs of support.

The legitimacy of this kind of governance broke down, however, as rapid social and economic change, together with the traumatic effects of the state's over-extension—brought to a focus with the fiscal crisis accompanying the depression of 1839–43—forced a reconsideration of the political order. Through constitutional reform, reinforced by new legislative initiatives and against the background of judicial lawmaking, there occurred what Gunn terms a distinctive modernization of government, with administrative agencies strengthened at the expense of the legislature and with an emphasis on the classical virtues of rationalization and efficiency. At the same time, these new "multiple centers of decision making" had to cope with a democratization of authority in the form of the expanded direct ballot, with the emergence of the general incorporation regime that separated private corporations from their formerly quasi-public functions, and with an intensifying differentiation of private and public spheres (p. 256).

The debt to Willard Hurst and the historians of state government and the economy (Oscar Handlin and Mary Handlin, Milton Heath, Louis Hartz, and others) is evident. Whether Gunn carries his case successfully in contending that the developments of the 1840s and 1850s "drained political participation of much of its substantive meaning" (p. 257) requires widening the scope of inquiry much beyond the law-and-economy issues he discusses; surely the antislavery question did in fact give political participation new substantive meaning, and the party developments of the 1850s can hardly be understood if it were otherwise. On another point, too, some reservations are in order: in light of the well-known and well-studied record of New York in planning, financing, constructing and operating of the Erie Canal system—largest nonmilitary enterprise of antebellum American government—can one sustain an argument that the state's political order in the 1820s and 1830s "was fundamentally incapable of the for-

mulation and implementation of sustained, coordinated policy" (p. 61)? Gunn's view of early common law in New York also underestimates, as do most studies of law in this era, the working importance of positive regulatory doctrines and the development of a robust legal and constitutional theory of public rights.

Those caveats notwithstanding, Gunn has set down a profoundly interesting challenge to those who study antebellum party organization and leadership, political and legal doctrine, ethnocultural politics, and ideology. A similar examination of the political order in other states and in various regions, which will permit some systematic comparison, will tell how well Gunn's view of the New York example describes the generality of experience in antebellum American governance.

HARRY N. SCHEIBER
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MICHAEL ALLEN. *Western Rivermen, 1763–1861: Ohio and Mississippi Boatmen and the Myth of the Alligator Horse*. Baton Rouge: Louisiana State University Press. 1990. Pp. xiii, 261. \$25.00.

This wonderfully researched and delightfully written book at last provides a realistic portrait of the rivermen of early America. Michael Allen admits that his original inspiration was watching Davy Crockett and Mike Fink on TV as a boy. Years later he spent several seasons as a deckhand on board a Mississippi towboat. Perhaps this accounts for his empathy with the boatmen, evident on every page. Warned that there were no first-hand accounts from rivermen themselves, Allen visited forty libraries and archives throughout the Mississippi Valley, locating eighty previously unused diaries, memoirs, and sets of correspondence. These give the narrative a remarkable immediacy. He supplements these with a sophisticated reading of traveler's accounts, observations of shoresiders, contemporary newspapers, and samplings from the court and wharf records of river towns. His social history is essentially *sui generis*, because nothing comparable has been written. Finally, he contrasts the social history he reconstructs with the image of the riverman as "half horse, half alligator" in folklore, literature, and art.

Allen divides his history into two main eras. During the second half of the eighteenth century and the first quarter of the nineteenth century, the inland waters were dominated by flatboats. Access to markets for trans-Appalachian farmers was southward down the river, and the flatboatmen carried literally anything and everything. Although the stereotyped riverman was a Kentuckian, Allen finds that there were many more from the north side of the Ohio, and that French-Canadians were the single largest ethnic group, many of mixed Indian-European ancestry.

Although rivermen included “merchant-navigators”—men such as Moses Austin and the infamous James Wilkinson—the vast majority were young, unmarried, and poor. Allen stresses the hard work and loneliness, the filth, illness, violence, and alcoholism of their way of life. Women were considered bad luck on board, but they were a prominent part of the river world in shoreside taverns and whorehouses.

During the subsequent steamboat era, from the 1820s to the Civil War, flatboating boomed. The number of boatmen on the rivers at any one time, he estimates, rose from about three thousand in the first era to over twenty thousand in the second. River improvement, better ships, and the ability to ride rather than walk the return trip upriver vastly improved the lives of rivermen. There were many more merchants and professional pilots, and they were frequently responsible family men. The common boatmen tended to be mere boys, like Abraham Lincoln, who made such a trip in the 1830s. During this period the river towns cleaned up their red-light districts, instituted taxation and licensing, and the whole way of life became more civil and far less risky, until it became antiquated by the dominance of rail transport after the war.

It was during this era that the nostalgic image of the “Alligator Horse” developed. It was manufactured from half-truths about the earlier period by popular culture figures such as Timothy Flint, Thomas Bangs Thorpe, the anonymous writers of the Davy Crockett almanacs, and, somewhat later, Mark Twain. George Caleb Bingham provided defining pictorial images. The central figure in this mythology was Mike Fink, whom Allen shows to have been a pitiful character in real life. But there was more than simply top-down manipulation of images here, for, as Allen demonstrates, those boyish boatmen eagerly promoted the stereotype in the interests of their own image and egos. The dialectic between the popular and elite uses of the riverman image remains somewhat underdeveloped in the book, but that is a minor criticism. Read this book to learn about an American original, and read it for pleasure.

JOHN MACK FARAGHER
Mount Holyoke College

HARRY P. OWENS. *Steamboats and the Cotton Economy: River Trade in the Yazoo-Mississippi Delta*. Jackson: University Press of Mississippi. 1990. Pp. xiii, 255. \$30.00.

“In a business point of view, this river is beginning to run up stream.” So wrote Thurlow Weed, New York Democratic Party wheel horse, in 1854 about the Mississippi River. The maturing of the river steamboat and the coming of the first railroads from the Northeast and the Midwest had already begun what some have called the “takeoff” period in American economic development and tied the South to the

North with bonds far stronger than any sectional similarities could. Transportation costs began to decline, as they would continue to do into the twentieth century, and the supply of cotton grew rapidly to meet a growing world demand. The new technology, railroads, would not finally complete a comprehensive national inland transportation system that, almost literally, went everywhere until the 1880s and, in the South, until early in the next century. Meanwhile, the demand for steamboats to carry the cotton out of the interior and up the mighty river to the railheads would sustain a golden era of the paddle-wheel steamer as new, more remote lands were being brought under cultivation. The old technology, steamboats (which had themselves been around for only twenty-five or thirty years by Thurlow Weed’s visit), would have to do.

How steamboats were coaxed to “bring out the cotton” from once-remote points amid some of the richest alluvial soils in the world, the Yazoo-Mississippi Delta, is the subject of Harry P. Owens’s fascinating book. In this case, the delta meant the complex of primarily north-south rivers, creeks, bayous, and swamps in the region roughly between the Mississippi River on the west and the mainline of the Illinois Central Railroad on the east, and between the latitudes of Memphis on the north and Vicksburg on the south, both major early ports on the Mississippi’s east bank. The most useful of these streams, the Yazoo, accepts the flow of most of the lesser streams in this delta before it flows into the Mississippi just above Vicksburg.

The first steamboats began to make their way up the Yazoo in the mid-1820s, but until the “magic” 1850s, most of these boats plied the Mississippi. Beginning in the 1850s, a vigorous competition arose between independent steamboat companies, headed by pilot-captain-entrepreneurs cast in the most romantic of molds, which centered their operations in Yazoo City, roughly halfway between Greenwood and Vicksburg. The companies brought more than a modicum of economic order and business system to the essentially rough-and-tumble steamboat business. Owens takes Captain S. H. Parisot of the “P Line,” who had some claim to being the most prominent of the Yazoo steamboatmen, as his chief example of the men who provided this service that was so vital to interior farmers until the end of the century—the most golden part of the “golden age” being 1870 to 1890. By that time the Illinois Central Railroad, to the virtual exclusion of any other railroad interests, dominated transportation by operating several north-south lines.

Owens is a well-known authority on this kind of steamboating, and his book is based on impressive research, primarily in government records and contemporary newspapers. Most readers will probably find the second half of his book more interesting than the first, for it tells us something about what these boats carried: not only cotton but also cottonseed,

lumber, and general merchandise; floating sawmills and cotton gins, which often operated right at the plantation landing; and sometimes even a compress that made low-density country cotton bales on the spot.

The first half of Owens's book, however useful to specialists, commits Allan Nevins's cardinal sin of "going from the particular to the particular," in this case the particulars being the names of countless boats, nearly all of which seem to have ended up, in fairly short order, burnt, sunk by snags or collisions, or sent to the bottom by Union gunboats during the Civil War. What I missed most of all was any discussion of the wider economic and social significance of the interior steamboats, which, after all, were only the first link in a much longer marketing chain. (In the 1860s, the first shipment of cotton arrived at Atlantic ports and New England textile mills by rail, having traveled as far as Louisville by river steamer. Some of this cotton may well have come down the Yazoo.) But a good book not only tells us much that we did not know but also makes us want to know even more, and Owens's is a very good book. Anyone interested in southern history or the rise of American material civilization should read it.

ALBRO MARTIN
EMERITUS
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HANS L. TREFOUSSE. *Andrew Johnson: A Biography*. New York: W. W. Norton. 1989. Pp. 463. \$25.00.

Andrew Johnson is one of the most controversial figures in American history. Like Abraham Lincoln, Johnson was a self-made man who started life in a log cabin. Unlike Lincoln, his presidency was a disaster. Johnson's failure was not caused by inexperience. On the contrary, his long career in Tennessee and Washington gave him impressive credentials. Beginning as an alderman and mayor in Greeneville, he served in both houses of the Tennessee legislature, was a congressman, governor, United States senator, military governor of Tennessee, and vice president. Yet, within eighteen months of becoming president, Johnson had failed at Presidential Reconstruction and totally estranged himself from the Republican Party that had nominated him as Lincoln's running mate. In 1868, in the only presidential impeachment trial in the nation's history, Johnson thrice came within a single vote of conviction.

Hans L. Trefousse, the biographer of Carl Schurz and other Republican leaders, takes Johnson from his tailor shop in Greeneville to national leadership. Early on, Johnson emerged as an agrarian radical and a disciple of Thomas Jefferson and Andrew Jackson, unencumbered with either's flexibility. Johnson's extremism made him a political maverick in the prewar Democratic Party and served him poorly as a Reconstruction leader. Similarly, his vituperative political

style, so effective in the Tennessee mountains, seemed in a president undignified to postwar northern voters. A champion of the common man, Johnson's democracy was of the *Herrenvolk* variety. He had owned house slaves and believed unequivocally in black inferiority; his was the poor white's virulent hostility to free blacks. Frederick Douglass recalled meeting him on inauguration day in 1865: "Mr. Lincoln touched Mr. Johnson and pointed me out to him. The first expression which came to his face, and which I think was the true index of his heart, was one of bitter contempt and aversion." Douglass shortly turned to a black companion and said, "Whatever Andrew Johnson may be, he certainly is no friend of our race" (*Life and Times of Frederick Douglass* [1892, 1962], p. 364).

More so than Lincoln, Johnson believed that Reconstruction was a job for the president. He was also rigid in his conviction that the southern states had never left the Union. These views, Trefousse believes, combined with unconcern for the freedmen, largely explain the Presidential Reconstruction plan that Johnson adopted after Lincoln's death. Harder to fathom is Johnson's persistence in a course that alienated the Republican majority in Congress and destroyed his presidency after his program's rejection. Herein lies the nub of the Johnson "enigma" (p. 13). Trefousse aligns himself with those scholars who believe that the seventeenth president was attempting to bring about a political reorganization and form a new party. If so, this also proved evanescent. Johnson ended up virtually a man without a party.

Trefousse relies extensively on new materials made available by the University of Tennessee's publication of the Andrew Johnson papers. Although this book does not fundamentally alter our view of the tailor-president, it presents an engaging synthesis of modern scholarship that supersedes previous biographies.

TED TUNNELL
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MICHAEL R. HYMAN. *The Anti-Redeemers: Hill-Country Political Dissenters in the Lower South from Redemption to Populism*. Baton Rouge: Louisiana State University Press. 1990. Pp. 252. Cloth \$39.95, paper \$16.95.

Michael R. Hyman reasserts a "staple of southern historiography," the existence of a dissident tradition in the upcountry, from antebellum Jacksonianism, through Independent and Greenback agitation in the 1870s and 1880s, and culminating in the Populist revolt (p. 200). He finds that powerful independent movements coalesced in Georgia and Alabama but only to a lesser extent in Mississippi. Hyman contends that "areas that suffered from the socioeconomic changes of the postwar decades witnessed significant defections from the Democracy" (p. 17). Whether as "Independent Democrats" claiming some loyalty to

the party or as opposition "Greenbackers," upcountry farmers chafed under Redeemer rule.

The insurgents whom Hyman terms "Anti-Redeemers" emerge from this account as coherent critics of the Democratic Party. As the designation "Greenbackers" indicates, the hard money proclivities of the national Democrats antagonized many small farmers, but there were a host of local issues too. Aggressive participants in the overthrow of Reconstruction, upland farmers favored massive cutbacks in government expenditures. But, while subsidies for business offended them, hill-country agrarians looked to government to control the merchant elite. Support for usury legislation was "widespread throughout the Georgia upcountry in the 1870s" (p. 43). Similarly, in Alabama, Anti-Redeemers in the legislature frequently opposed Democratic priorities. For example, in the early 1880s they resisted anti-enticement laws and generally dissented from authoritarian labor legislation (pp. 217–18). They opposed the Democrats "on issues ranging from government spending, to revision of the state election laws, to state interference with the selection of local officials" (p. 83). The limited practical effects of their efforts drove the Anti-Redeemers toward the more drastic solutions later proposed by the Populists.

Hyman argues that circumstances often forced Anti-Redeemers to defend African-American interests. For example, they militantly opposed the convict-lease labor system as well as electoral fraud. "Calling for a 'free ballot and a fair count,' Anti-Redeemers condemned the electoral chicanery practiced by the Redeemers" (p. 182). For these reasons, they secured substantial numbers of black votes for their candidates at the local level, especially "in hill-country areas where opposition movements were the strongest and economic and physical intimidation by landlords was not as severe as in black belt counties" (p. 186). But racist traditions bedeviled attempts to broaden the opposition ranks. Open fusion with the Republicans at the state level proved difficult, for it enabled the Democrats to reawaken memories of Reconstruction with telling effect.

Hyman's account is not uniformly persuasive. For example, his argument that the speed of commercialization explains the strength of Anti-Redeemer sentiment seems too schematic. The dimensions and precise form of opposition movements in the upcountry seem somewhat random. In general, however, this work's positive but balanced portrayal of his subject seems impressive. In recent years some scholars, most notably Barton C. Shaw and Charles L. Flynn, have argued that upcountry Anti-Redeemer strength did not presage the Populist revolt. Those holding such views will now have to take this well-researched rebuttal into account.

MICHAEL W. FITZGERALD
St. Olaf College

CAROL REARDON. *Soldiers and Scholars: The U.S. Army and the Uses of Military History, 1865–1920*. (Modern War Studies.) Lawrence: University Press of Kansas. 1990. Pp. viii, 270. \$35.00.

The defeat of Iraq in Operation Desert Storm offers an index of just how alive and well military history is in the United States Army today. When U.S. troops deployed to Saudi Arabia, they were accompanied by officers, many of them holding advanced degrees in history from major universities, specifically assigned to record the history of the undertaking. This effort was made possible by the extensive investment that the army has made over the past thirty years to insure a steady supply of line officers who are also university-educated historians to fill the many faculty positions requiring competence in military history throughout the professional military education system. In short, the U.S. Army today believes in military history and backs that belief with substantial funding. As the Superintendent of the Military Academy stated at a recent meeting of the American Military Institute, one of the educational objectives of the army is to make the officer corps "history minded." Unfortunately, this has not always been the case.

Carol Reardon's study traces the vicissitudes in the army's belief in the utility of military history from the end of the Civil War to the end of World War I. Many nineteenth-century officers were skeptical as to the value of military history, believing that only the hard school of experience could instruct one in the military art. But as the Civil War receded in time, history in lieu of experience began to acquire uniformed advocates. The national trend toward professionalism led the army to develop postgraduate schools and post-lyceums. But the process was exceedingly halting.

In the gradual advance toward professionalism, a few names stand out of young officers whose use of history in the army schools had a significant impact. Arthur L. Wagner and Eben Swift, who argued that "principles are best learned by their application rather than by the abstract study of the principles themselves" (pp. 38–39), developed the applicatory method at Fort Leavenworth. This was a long step forward. The method used history to teach order-writing and doctrine, but it was deductive and based on historical examples selected to demonstrate a priori principles. In sharp contrast was the approach of Arthur L. Conger, who developed the source method of study using primary materials.

There was no "school solution" in his teaching, which reflected the research methods of civilian universities. The goals were objectivity and analysis and teaching officers to think for themselves. Conger's methods at the staff school helped mold many of the leading generals of the American Expeditionary Forces in World War I. Sadly, however, the army failed to insure adequately educated faculty members to introduce Conger's sophisticated source method of teaching throughout the army school system. The use

of history largely reverted to the deductive pattern of Swift and Wagner until reforms in the 1960s. One can only hope that Reardon will write another volume carrying her analysis down to the seemingly more historical-minded present to show how and why this transformation came about.

I. B. HOLLEY, JR.
Duke University

HAROLD M. HYMAN. *Oleander Odyssey: The Kempners of Galveston, Texas, 1854–1980s*. (Kenneth E. Montague Series in Oil and Business History, number 6.) College Station: Texas A&M University Press. 1990. Pp. xxii, 486. \$39.95.

Harris Kempner, according to Harold M. Hyman, realized in full measure the American dream. He arrived in New York in 1854, an almost penniless seventeen-year-old refugee from anti-Semitism and conscription in Russian Poland. Four decades later he died in Galveston, Texas, a millionaire and founder of a dynasty. The work ethic served him well. As a laborer in the Northeast, he saved his money, then moved to a rural Texas community and became first a peddler and then a merchant. Resettling in Galveston, he established himself as a wholesaler and cotton factor.

Kempner died at a relatively young age, but he had married well. His widow joined with their eldest son Ike, then barely twenty-one years old, to rear eight younger children and continue the business. During the long tenures of mother and son—both lived into their nineties—the family unit remained intact and the business expanded into insurance, banking, sugar, town building, and international trade.

The most dramatic part of the Kempner saga deals with the Galveston storm of 1900, one of the worst natural disasters to hit an American city. In its wake, young Ike proved his mettle as he fought to save the city from extinction and the family from losses. To insure the future of the island, he led in the building of a sea wall and causeway and in raising the elevation of the city. He also sponsored reform in the corrupt city government by advocating the adoption of a commission plan, the Galveston Plan of the Progressives.

The theme of the book is the acculturation of a southern Jewish family. Harris Kempner found a generally open society to which he responded wholeheartedly, even to shedding blood in the Confederate Army. "I came to America to be an American" (p. 236), he wrote in 1893. The family followed his lead, but acculturation was probably more complete than he envisioned. In the second generation, family members began marrying outside his faith and, as Ike Kempner noted, in the next generation they married into Baptist, Episcopalian, and Roman Catholic families. Before Ike died in 1967, he observed wistfully

that none of his grandchildren went through the bar mitzvah or was confirmed in synagogue ritual.

This is an authorized history, based on family archives and written with cooperation from the family. Thus, Hyman possibly overstates the altruism of the Kempners and glosses over a few aspects of their story. For example, he stresses their influence—economic, social, and political—in Galveston, but at the same time he absolves them from responsibility for the vice and lawlessness that made the city notorious in the decades following World War I.

Even so, this book has both depth and breadth. No mere rags-to-riches story, it traces the economic and social evolution of a region over a long period of time. It also gives perceptive insights into unionism, convict labor, company town building, and Jewish philosophies and attitudes toward immigration.

MARILYN McADAMS SIBLEY
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JEREMY W. KILAR. *Michigan's Lumbertowns: Lumbermen and Laborers in Saginaw, Bay City, and Muskegon, 1870–1905*. (Great Lakes Books.) Detroit: Wayne State University Press. 1990. Pp. 361.

THEODORE J. KARAMANSKI. *Deep Woods Frontier: A History of Logging in Northern Michigan*. (Great Lakes Books.) Detroit: Wayne State University Press. 1989. Pp. 305.

By the 1870s, the twin communities of Saginaw and Bay City, Michigan, had become the nation's—and probably the world's—leading lumber-producing center. Other towns in the state also soon rose to importance. But in spite of its key place in the history of American lumbering, Michigan's forest industry has been little studied. The works by Jeremy W. Kilar and Theodore J. Karamanski are important correctives. Together with Barbara E. Benson's *Logs and Lumber* (1989), which treats logging in Michigan up to 1870, these books provide comprehensive coverage of lumbering in one of the industry's key states.

Kilar focuses on Saginaw, Bay City, and Muskegon during the years 1870–1905, tracing the development of pine lumbering in lower Michigan; Karamanski details the evolution of forest industries in the Upper Peninsula. Their approaches, however, are decidedly different. Kilar emphasizes such issues as ethnic and class divisions and absentee versus local ownership in explaining the differing courses of events in the three communities. In the process, he draws heavily on the new social history. More traditional, Karamanski stresses business history and conditions faced by loggers and lumbermen; but he goes beyond the era of pine logging, to which Kilar confines himself, to examine the subsequent—and important—periods of hardwood and pulpwood production.

Kilar shows that lumbering followed a different course in each of the three towns he studies. In Saginaw the locally based leadership had sufficient

dedication to the community and enough in common with its working class to ease it through the difficult transition period when pine lumbering was ending and a new industrial base was developing. In Muskegon the ethnic mix of the labor force, which included a sizeable portion of conservative Dutch residents, moderated class conflict and undercut the efforts of union organizers. In Bay City absentee owners such as Henry W. Sage had only superficial interest in the community; they contributed little to social overhead investments, threatened to close their mills and move elsewhere when challenged by organized labor, and, as accessible stands of pine disappeared, did transfer their operations to other regions. Moreover, in Bay City the working class included many Poles, who combined strong traditions of class consciousness and labor activism with a deep-seated clannishness and sense of alienation. This contributed as much as the community's industrial leadership to Bay City's failure to move from almost total dependence on lumbering to a broader industrial base.

Karamanski examines the rise of the pine industry of the Upper Peninsula, highlighting the role of individual business leaders and how the advent of railroads to the interior modified their long-time dependence on water transportation, especially log drives on the area's rivers. He also details the technology of logging and the sharply contrasting social environments of loggers and lumbermen. All this is effectively presented, but contains little that is new. When Karamanski considers the hardwood and pulpwood eras, however, he is pioneering. His emphases remain the same—leadership, technology, spatial relationships, and the social environment—but by juxtaposing the pine, hardwood, and pulpwood industries, these familiar approaches take on an unexpected freshness. Especially welcome are Karamanski's discussions of the advent of industrial forestry in the Upper Peninsula (largely through the efforts of Henry Ford), of the reasons for the widespread failure of the area's twentieth-century operators to adopt sustained-yield forest management practices, and of the interrelationships between the mining and timber industries of the peninsula.

Although more intellectually ambitious, Kilar's study is less satisfactory than Karamanski's. Kilar's work is replete with assumptions and generalizations not grounded in his evidence; the applicability of works in labor and ethnic history, which Kilar draws on to support his central theses, is often inadequately demonstrated; and repetitiveness, vagueness, and awkward syntax abound. By sticking to more familiar intellectual terrain, Karamanski risks less yet produces a more satisfactory result.

Neither work is apt to become a classic, but both are valuable. Whatever his failings, Kilar offers fresh approaches to forest history that should help to bring the field closer to the profession's cutting edge. And by taking the story of lumbering in Michigan beyond

the era of pine, Karamanski makes a useful contribution to forest history, which all too often treats the story of lumbering in America as one of softwood production and nothing more.

THOMAS R. COX
San Diego State University

ROBERT DOHERTY. *Disputed Waters: Native Americans and the Great Lakes Fishery*. Lexington: University Press of Kentucky. 1990. Pp. viii, 172. \$24.00.

Robert Doherty's book is a history of sport and commercial fisheries in those portions of lakes Michigan, Superior, and Huron bordering the state of Michigan. Although the author's main interest is the attempt by the Chippewa and Ottawa Indians to reassert treaty rights to fish these waters commercially without state supervision, a crusade that began in earnest in the 1960s, he also includes sections on the early fur trade, the rise of upper-class sport hunting and angling in the nineteenth century, and the arrival of growing numbers of middle-class tourists in recent decades.

As the dust jacket proudly proclaims, Doherty is "not afraid to take the side of what . . . [he] perceives as an oppressed minority group and to make policy recommendations to correct injustice." From the start, therefore, some historians may wonder if this monograph has been written more as a tract on behalf of Indian rights than as an effort at objective scholarship.

There is, of course, no reason why a professor of history should not become an activist and join the struggle for a worthy goal. But, in taking up this cause and writing a book that would further it, Doherty has not always been completely fair with those opposing the Native American campaign to regain treaty rights.

In the fight to fish commercially without state interference, Indians have found that their chief adversaries are sport anglers and the Michigan Department of Natural Resources. Doherty has little good to say about either. Even fellow outdoorsmen of an earlier era, sport hunters, are treated as cynical, self-interested competitors with Native Americans. As Doherty argues, "sportsmen claimed that anti-market-hunting laws were in the public interest, but that hardly seems the case," for "elimination of commercial hunting merely allocated wildlife from one social group to another" (p. 49). Furthermore, "sportsmen simply made their interest and the public interest *appear* [emphasis added] to be the same, and state regulation of wildlife served that part of the public interest represented by upper-class hunters" (p. 49).

For Doherty, the human use of wildlife, and in fact all of nature, seems to be merely a matter of "allocation," a term he employs repeatedly. That sport hunters in the last century, or sport fishermen recently, could possess what I have called the "code of

the sportsman," which defines market hunting and fishing as unethical and even immoral, is understood poorly, if at all. He describes incidents of white anglers harassing Indian net fishermen because sportsmen felt deeply about unfair fishing methods that impeded the state's efforts to restore species previously depleted by unregulated netting. Yet, according to Doherty, virtually all concerned whites, "administrators, politicians, and leaders of sportsmen's groups . . . [merely] used anti-Indian feeling to advance their careers" (p. 153); supposedly, they possessed no higher motive beyond self-aggrandizement.

This study may prove to be a valuable historical reference in future litigation regarding Michigan's fisheries, especially on the Native American side of the argument. But, because of its partisan viewpoint, the book will be less useful to scholars trying to understand why white resistance to Indian claims has been so impassioned.

JOHN F. REIGER
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JAMES EARL SHEROW. *Watering the Valley: Development along the High Plains Arkansas River, 1870–1950*. (Development of Western Resources.) Lawrence: University Press of Kansas. 1990. Pp. xiii, 222. \$29.95.

James Earl Sherow has written a very good book. Soundly grounding his study in the earlier work on the history of water resources, Sherow adds the "environmental factor" to a discussion of irrigation, industrial, and suburban demands for water in the so-called "Valley of Contentment" (p. 1), the watershed of the upper Arkansas River. The author chronicles the initiation of small-scale irrigation around Rocky Ford, some forty miles below present-day Pueblo, Colorado, under the leadership of George Swink, the indefatigable promoter of beets, cantaloupes, honey, alfalfa, and other water-demanding exotic crops. In the best nineteenth-century mode of making the dry land bloom, Swink and others downstream switched permanently from livestock and hay along Colorado's section of the Arkansas River to a cash-crop economy. After describing the truck garden complex of the Rocky Ford Ditch, Sherow moves to the Fort Lyon Canal and the Bessemer Irrigation Ditch Company, other agrarian interests and their promoters around the turn of the present century. He then investigates urban thirst for water, typified by the city of Pueblo, and concludes with the interstate squabble between Colorado and Kansas. Kansas was the ultimate loser in the fight for riverine supplies.

What are the lessons from this story? Sherow allies himself with the finite planet school. Swink exemplified the urge toward creative capitalism through innovation and promotion. Once water is regarded as

merely a commodity, albeit a vital one in that dry region, the fabric of the environment—its soils and native flora and fauna—is of little account. Transforming the riparian landscape into cropland, overplanting with new crops, and attracting more people intensified pressures for abundant, high-quality water. Social conflict results, especially when nature is tardy or niggardly in seasonal replenishment. Even the engineers, scarcely disinterested players in water development, cannot solve the supply problem.

Sherow's well-crafted portrait of this river is familiar. The resource has a unidimensional value; water is something to be hoarded, purchased, or exchanged without regard for its importance for indigenous plants, animals, and people who learned to adjust to fluctuations in runoff and timing. What results is an overbuilt landscape that reflects a lack of concern for scale and an intolerance of limits. Sherow tells us how and why this has happened along the upper Arkansas and why living within constraints makes for good ecology and ultimately for good business.

Forty pages of notes and sources provide an important grounding for this work. The first two base maps of the entire study area are poorly rendered. They omit some communities mentioned in the text, including Fort Reynolds, Fort Lyon, and Fort Larned. Other places should be specifically keyed to illustrations. Such omissions, however, do not mar a carefully crafted and scholarly work that illuminates the important issue of water "control" and planning.

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University of Texas,
Austin

SAMUEL H. PRESTON and MICHAEL R. HAINES. *Fatal Years: Child Mortality in Late Nineteenth-Century America*. (NBER Series on Long-term Factors in Economic Development.) Princeton: Princeton University Press. 1991. Pp. xxi, 266. \$37.50.

This book by Samuel H. Preston and Michael R. Haines examines child mortality in late-nineteenth-century America based on a sample from the 1900 census. The study comprises five chapters. Chapter 1 discusses major factors previously used to explain variations in mortality across groups and places. These variations range from aggregate influences such as urbanization and the quality of water and milk supplies to individual-level determinants such as race, ethnicity, income, literacy, and breast-feeding practices.

Chapter 2 introduces the Public Use Sample of the 1900 census and reports on methods used to estimate child mortality from it. The authors discuss their indirect estimation technique (the "surviving-children" method) in detail and carefully compare their results to those yielded by other methods, and to figures from the well-known Glover Life Tables. They find that the Glover tables, based on the mor-

tality experience of the 1900 Death Registration Area (DRA), provide an overestimate of African-American mortality, but a rather accurate estimate of American mortality as a whole. Chapter 3 explores mortality determinants one by one, revealing results fairly similar to previous studies. Mortality was higher for children of African Americans and foreign-born women. Children of illiterate and urban women also had relatively high rates.

The most important findings, however, appear in chapter 4, in the results of the multivariate analysis. Here, racial variation is the most critical determinant, followed by the size of the child's community. Although child mortality did not increase monotonically with city size, larger places tended to be more unhealthy. By contrast, variables such as mother's literacy, father's occupation, and place of birth (native or foreign) wane in importance in the multivariate analysis.

Chapter 5 compares U.S. child mortality with contemporaneous English rates and shows that variations in child mortality across social classes were more salient there. Preston and Haines note, however, that "what occupational class was to England, race was to the United States" (p. 198). The authors also compare U.S. figures to a sample of data from the present Third World to support their main conclusion—that parental ignorance, stemming from a lack of medical knowledge, was the single most important cause of relatively high child mortality in the United States in 1900. Economic factors, they argue, are inadequate for explaining high levels of child mortality, since it is clear that knowledge gained after 1900 has led to massive reductions in child mortality in societies significantly poorer than the United States was in 1900.

This conclusion is doubtless well founded but leads attention away from one of the book's most consistent findings about child mortality in the United States in 1900, namely the importance of racial differences, particularly in America's cities. Other influences were surely more critical within particular local and regional settings, but these paled in importance when the nation was considered as a whole.

Preston and Haines's fine study is written with an interdisciplinary audience in mind. Although some of the methodological discussions will be beyond the grasp of certain readers, the logic and general clarity of their presentation will allow most to follow their major arguments.

KATHERINE A. LYNCH
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MARK I. WEST. *Children, Culture, and Controversy*. Hamden, Conn.: Archon of Shoe String. 1988. Pp. x, 130. \$23.50.

Mark I. West deals with the responses of various individuals and groups during the last century to media forms directed at children. The negative re-

sponses to these forms, ranging from Anthony Comstock's nineteenth-century crusade against dime novels to the New Right's objections to rock and roll and the secularism of modern textbooks, are presented by West in ten short, well-written chapters. The major problem is that West's book is devoid of a cohesive, explicit thesis.

The introductory chapter maintains that the idea of childhood innocence enjoyed increasing popularity during the nineteenth century, but West never states that the notion of innocence is the thread that binds this book. Worse, he makes the assumption that since "the history of childhood is really a history of ideas" (an unexamined notion), it follows that when writers' ideas about childhood have changed so have the "characteristics of American children's culture" (p. 1). There is, however, no necessary connection between, for example, what writers of child-rearing manuals maintain and the actual behavior of parents who read these books toward their children.

This leap of logic is characteristic of much of the book. For instance, West claims with some validity that Comstock's objections to the material in dime novels was tied to his lifelong sexual anxieties; it is quite another matter to claim, as West does, that the same fears motivated "many of the people" who supported Comstock (p. 19). Or again, West states that the librarians of the late nineteenth and early twentieth centuries who attempted to censor children's books were mainly unmarried women. Lacking close contact with children, they could maintain that children were possessed of an innate innocence until corrupted by society. Since the eighteenth-century French philosopher Jean-Jacques Rousseau also "was never involved with raising children" (p. 30) and believed that they were cisterns of primordial innocence, West argues, it was only natural that he influenced nineteenth-century American librarians.

The tendency to twist complex skeins of thought and behavior into the neatly packaged notion that modern critics of children's media premise their objections on an unstated commitment to the idea of childhood innocence reaches an unfortunate crescendo in West's analysis of the contemporary religious New Right and its crusade against books, textbooks, and television programs aimed at children. Do all segments of the complex and uneasy alliances that constitute the New Right truly maintain that human beings are born without the stain of Adam and Eve's fall on their souls? Too many flawed arguments like this mar what could have been a most valuable addition to the historical literature on modern childhood.

DOMINICK CAVALLO
Adelphi University

EDITH NYE MACMULLEN. *In the Cause of True Education: Henry Barnard and Nineteenth-Century School Re-*

form. New Haven: Yale University Press. 1991. Pp. xii, 378. \$37.50.

In 1965, the preeminent historian of American education, Lawrence A. Cremin, noted the dearth of biographies of important educators. Indeed, at the time of his death in 1990, he was working on a biography of John Dewey. Several of his students have attempted to fill this void. Edith Nye MacMullen's well-researched book is unquestionably one of the "detailed, critical biographies" Cremin called for, and she acknowledges his guidance. Both agree that Henry Barnard's role in the formative history of modern, free public education—the "common school movement" in antebellum America—needs deflating. Cremin's trilogy, *American Education* (1970, 1980, 1988), merely lists Barnard among state leaders who attempted to put public education on a secure financial basis, improve schoolhouses, promote teacher education, extend the school year, make the curriculum uniform, and introduce pedagogical and disciplinary reforms. For her part, MacMullen's biography could be subtitled "Henry Barnard, Mythmaker," so much does she take apparent delight in exposing Barnard's own efforts to write himself into historical immortality as second only to Horace Mann as the "Father" of public education in the United States.

Henry Barnard (1811–1900), son of a prosperous and indulgent Hartford sea captain and investor, graduated from Yale, taught school briefly and unsuccessfully, practiced law, and was elected to the Connecticut legislature at age twenty-six, joining an already active campaign for better common schools in 1838. He was, successively, Connecticut's first state superintendent of education, Rhode Island's commissioner of public schools, state superintendent and principal of Connecticut's new normal school, chancellor of the University of Wisconsin, president of St. John's College, and first U.S. commissioner of education. He retired to Hartford in 1870, continuing a long career in educational writing, editing, and publishing, producing thirty-two massive volumes of *The American Journal of Education* between 1855 and 1881. This latter work is the achievement that MacMullen judges Barnard's own permanent and deserved claim to fame. His other contributions are diminished, "flawed," or begrudged in this portrait of Barnard: an "indefatigable" worker of "prodigious energy" (p. 68), yet a "gentlemanly and leisurely reformer" (p. 85).

Insensitive, ungenerous, hypochondriacal, self-serving, unreliable, dissembling, long on intent and rhetorical promise but short on solid achievements, exploitative, sexist, racist, administratively inept, politically clumsy, often original, opportunistic—this litany of Barnard's character defects contrasts oddly with the life-long devotion of countless loyal friends and associates. That so many of his contemporaries could have been so persistently deceived for so long strains credulity.

"I will not make my tiger a cat to please anybody," insisted James Boswell, Samuel Johnson's biographer; but he valued both his subject's faults and merits. Or consider John Richardson's biography of Picasso (*A Life of Picasso* [1991]), a proficient myth-maker about his own life. Richardson corrected without diminishing and probed for understanding in the intriguing complexities and contradictions of his subject. MacMullen's work is a "warts and all" portrait; certainly not hagiography, neither is it demonological. But the effect is an unsatisfying debunking that raises more questions than it answers. One closes this book sympathizing for the subject and hoping that he will someday find another biographer, one who enables the reader to see both into the person and, in Thomas Carlyle's words, to see "out of him, to view the world altogether as he viewed it." Despite MacMullen's impressive scholarship, this biographical goal is unmet. This, then, is not quite the definitive life of Barnard despite the solid contribution that she makes to the postrevisionist historiography of the common school.

GERALDINE JONCICH CLIFFORD
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MARJORIE MURPHY. *Blackboard Unions: The AFT and the NEA, 1900–1980*. Ithaca: Cornell University Press. 1990. Pp. xiii, 284. \$28.95.

This is an ambitious book in both conception and execution. Marjorie Murphy's study not only provides the reader with the institutional histories of the American Federation of Teachers (AFT) and the National Education Association (NEA) but also discusses how the two groups interacted with other social developments of the twentieth century. In doing so she furnishes a far more nuanced picture than would have been given by a more narrow institutional story.

Murphy fittingly begins her story with Chicago teachers at the turn of the century. In a city known for its support of labor, female elementary teachers, many of them from working-class origins and virtually all of them deeply rooted in the communities in which they taught, turned to the labor movement when confronted by administrators' attempts to centralize the school system and professionalize teachers. The NEA, dominated by school administrators and male high-school teachers, embraced the goals of centralization and professionalization. At the same time, some of the NEA's affiliates began to emphasize salary issues, thus giving more weight to community ties and political organizing. Murphy discusses the ways in which gender dynamics simultaneously reinforced and complicated this nascent distinction. Explicitly feminist classroom teachers, looked at askance by the male leadership of the NEA, found themselves welcomed only warily by parts of the labor movement.

These basic themes run throughout Murphy's story of the two organizations. By the 1920s the NEA would emerge as the major professional organization of teachers, while the AFT was fully accepted neither by the labor movement nor by many of the nation's teachers. During the devastation of the Great Depression neither organization came up with more than piecemeal responses to school closings, "payless pay-days," and all-out bankruptcy of some school districts. The AFT, furthermore, became embroiled in what Murphy designates as a "generational conflict" between an older Progressive leadership and a younger group of Communists (p. 152). By the 1950s political developments would force both the AFT and the NEA to carry on battles in support of academic freedom and tenure policies for teachers. Increasingly, any support for community concerns was defined as politically suspect. The resulting narrowed focus in both organizations completed the separation of teachers from the communities in which they taught.

During the 1960s the AFT moved first to consider the implications of collective bargaining for teachers. As the Federation dealt with strikes and bargaining, the NEA was drawn into the realm of union demands as well. True to its activist tradition, the AFT began the 1960s by leading the NEA in fights against the segregated school systems of the South. By the late 1960s, however, the Ocean Hill-Brownsville dispute in New York City would highlight the possible conflict between the demands of collective bargaining and calls for community control of the schools. The NEA, with its first black woman president, could now emerge as the more progressive of the two organizations while still making its traditional claims to professionalism.

Murphy concludes her excellent study by arguing that the tensions between the two organizations continue to play themselves out against the backdrop of fiscal crisis. Her book thus compels us to consider the ironic position of teachers torn between community needs and their own collective concerns.

ILEEN A. DEVAULT
Cornell University

ANN FABIAN. *Card Sharps, Dream Books, and Bucket Shops: Gambling in Nineteenth-Century America*. Ithaca: Cornell University Press. 1990. Pp. xi, 250. \$24.95.

Readers should not judge this book by its cover. True, Ann Fabian does argue that the rise of market capitalism transformed gambling from harmless, fair, and friendly games into an impersonal business where operators won sizeable sums from unsuspecting players. And to illustrate the change, she sketches the card sharps, dream books, and bucket shops mentioned in the title. But her primary project is not analysis of gambling in nineteenth-century America

but consideration of how discussion of the activity helped to define economic virtue and rationality. Historians should find the effort more than ample compensation for the sleight-of-hand deployed in naming the book.

Fabian contends that nineteenth-century critiques of gambling teach us little about the world of bettors and much about the culture of market capitalism. The transition from a symmetrical, "intimate and reciprocal" economy, geared for "limited gain," to an impersonal one of "open and endless commercial markets," "designed to end in asymmetrical accumulations" (pp. 3-4, 38), prompted bourgeois Americans to crusade against gambling as subversive of the values of thrift, accumulation, and hard work associated with the "rational" pursuit of wealth. Antigambling never gained the popularity of antislavery or temperance, partly because its leading proponents included gamblers-turned-reformers who, in using their special fund of experience to denounce the evils of gambling, undermined their own credibility. Nonetheless, the crusade endured in various forms, in particular addressing workers, immigrants, and non-whites whose betting, Fabian writes, represented "a logical revolt against the rampant rationality of nineteenth-century business civilization" (p. 137).

Those who attacked gambling not only marked the path to economic virtue but also sanctioned such risky enterprises as investing in commodity futures. When Populists and others complained about brokers buying and selling wheat that did not exist, the Chicago Board of Trade defended commodities speculators in part by contrasting them to the "real" gambler at work in the demimonde of bucket shops, which served as shadowy commodities exchanges for the lower classes. One kind of speculation was labeled respectable and productive, the other illegitimate and destructive.

While this book provides fresh insights on the mentality of market capitalism in nineteenth-century America, readers may feel compelled to hedge a few bets. Fabian tends to organize the past around a series of unrealistically absolute distinctions. America's economy, its gambling, and its social groups, she contends, have been characterized by either symmetry, reciprocity, restraint, and community (either in the premarket era, or among workers, farmers, and minorities), or asymmetry, exploitation, excess, and strangers (among businessmen and professional gamblers in the market era). Implicit in this view are suggestions of a golden age torn asunder by modern capitalism and of a polarization of American culture into mutually exclusive spheres. Perhaps market rationality was not all that rampant. Moreover, Fabian does not always marshal a convincing amount of evidence to indicate that the attitudes she identifies were widely held—at least in part because the odds against finding such data are high. But it might be fitting that Fabian, inquiring into the nineteenth-

century roots of what is termed the "casino capitalism" of recent America (p. 11), must resort to speculation.

JOHN M. FINDLAY
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JAMES D. NORRIS. *Advertising and the Transformation of American Society, 1865–1920*. (Contributions in Economics and Economic History, number 110.) New York: Greenwood. 1990. Pp. xviii, 206.

Scholars and critics of advertising have long pondered an intractable conundrum. Does advertising shape values and behavior, or does it mirror them? Is it an agent or an index of social change? Like many who have studied the topic, James D. Norris has a hard time making up his mind.

In this attempt at an overview of advertising between the Civil War and World War I, Norris accuses historians of fearing, and thus overestimating, advertising's power over consumers. Yet by the end of this brief volume he has charged advertisers and agencies with a long train of abuses, from exercising "unbridled economic power to silence [press] criticism" (p. 167) to promoting the "endless and mindless pursuit of consumption [which] . . . has led us to an environmental abyss" (p. 169).

The book begins with a chapter on the economic setting, emphasizing development of the railroad network, population growth, westward migration, urbanization, and rising incomes. He looks at mail-order houses and department stores as new distribution institutions. Finally, he packs in comments on the marketing of soap, sewing machines, and farm implements.

Succeeding chapters have a similarly scattered quality. Thus, one entitled "Leisure Time for the Ladies: Bicycles, Cameras, Appliances and Other Luxuries" mixes together advertising in these disparate product categories along with ads for canned foods and cleaning products. He is unconvincing in claiming that recreational articles in general were targeted at women. Nor does he show that guilt was the main theme in advertising for female readers. His analysis of gender draws on almost no recent work in women's history.

Intermittently, Norris reiterates the sensible point economist Neil Borden made nearly half a century ago: advertising works best when it reinforces trends in taste and fashion but loses impact when it attempts to counter these underlying forces. His decisions about where to credit advertising with boosting sales seem arbitrary. Thus, for example, he finds tobacco advertising to have been highly successful, especially in inducing nonsmokers to take up the habit. He does not, however, refer to the work of Richard Tennant, who maintained that advertising in itself did little to boost cigarette sales, nor to Michael Schudson's contention that advertising trailed women's widespread

adoption of cigarette smoking. Similarly, in his final chapter, Norris concludes, "In selling the automobile, advertising and American industry came of age" (p. 144). Yet he concentrates on Henry Ford, who used little paid advertising and shunned the style and snob appeals that Norris considers prevalent by the 1910s.

Norris wrote a useful article on advertising history in 1971. Since then, unfortunately, he has not incorporated much of the burgeoning literature in cultural studies, political economy, and business history that bears on the development of advertising. Nor has he explored corporate archives or printed primary sources from the advertising industry. He frequently cites older general studies of advertising history by Frank Presbrey (*The History and Development of Advertising* [1929]) and James P. Wood (*The Story of Advertising* [1958]), but neither his conceptualization nor his research takes us far beyond these works. We still need an account of the formative years of advertising that integrates business and cultural history.

DANIEL POPE
University of Oregon

OLIVIER ZUNZ. *Making America Corporate, 1870–1920*. Chicago: University of Chicago Press. 1990. Pp. x, 267. \$24.95.

In this book, Olivier Zunz answers a call he issued in *Reliving the Past* (1985) for a social-historical study of the middle class. Inquiring about the sources of corporate culture in both senses—the culture of corporations such as DuPont and Ford and the larger culture characterized by what others have called corporate hegemony—this new book combines social history with business and cultural history.

Zunz studies the personnel records of five large corporations to get the names of turn-of-the-century "rank and file capitalists" (p. 6), salaried employees in the middle ranks of corporate bureaucracies. With the doggedness and the techniques of the local quantitative historian, Zunz uses newspapers, manuscript census records, city directories, private correspondence, and material from local genealogical societies and company archives to track these individuals and learn about their backgrounds, daily lives, and attitudes. He mentions Louis Brandeis, Frederick W. Taylor, and numerous DuPont relatives, but his central actors are obscure, such as E. T. Wolf, a top DuPont salesman, or A. E. Touzalin, who rose in the ranks of the Chicago, Burlington & Quincy Railroad (CB&Q) by bringing systematic procedure to its central land office in Iowa.

Zunz claims to be asking bold new questions outside the bounds of established debates and interpretations. His introduction cites Thorstein Veblen, C. Wright Mills, Richard Hofstadter, Helen Lynd, and Robert Lynd, and suggests that a politically inspired search for cultural resistance to capitalism has di-

verted historical attention from the middle class. But, in fact, there is a growing historical literature on middle-class work and consumption habits coming from such authors as Cindy Aron, Stuart Blumin, and Ileen DeVault. Zunz cites such historians in footnotes but rarely addresses them in his text.

Zunz does address the issues and categories that have emerged, on the one hand, from radical labor historians and, on the other hand, from Alfred Chandler and his followers. He owes much to both schools. His prose and conceptual framework are full of ideas such as resistance, cooperation, and cooptation, and Zunz raises gender issues as if everybody did. But he asserts that middle managers designed the corporation in response to their own daily problems and that corporations did not so much impose a new work culture as modify existing ones. Zunz also maintains that Chandler opened the door to the study of the bureaucracy and describes what Chandler slighted: the changes in social relations that went along with structural changes in the corporation. As if to drive the point home, Zunz uses more evidence from DuPont, the company Chandler studied most closely, than from the rest of the companies.

Zunz uses sources with flair and imagination, often bringing the reader into the archives. He analyzes not only the content but also the looks of a collection of CB&Q job application letters: the handwriting and typing. He comments on corporate image-making during an era often presumed to predate modern public relations by examining the McCormick letterhead, which in the wake of the labor unrest of the 1880s traded its image of the factory and smokestacks for one of a reaper on the Great Plains. He provides the complete text of an investigation by the Ford Sociology Department, both demonstrating his points about the employee in question and offering those who will never use the Ford archives a glimpse of the documents. And Zunz pulls his disparate evidence together with conceptual ties that cross the boundaries of such specialties as urban history, labor history, and architectural history.

SUSAN STRASSER
George Washington University

GREGORY W. BUSH. *Lord of Attention: Gerald Stanley Lee and the Crowd Metaphor in Industrializing America*. Amherst: University of Massachusetts Press. 1991. Pp. xv, 224. \$27.50.

In 1913, Gerald Stanley Lee, a former Congregationalist minister, issued *Crowds: A Moving-Picture of Democracy*. Twentieth-century business magnates and politicians, Lee insisted, needed to follow Jesus's example of the expressive personality by using the tools of advertising and public relations to establish their preeminence over the disorderly masses. For a time, Lee succeeded in realizing his own vision of the heroic leader as master of the people's attention: the

year it appeared, *Crowds* was the best-selling nonfiction book in the United States. Gregory W. Bush has provided an insightful study of Lee's career, focusing on the way in which Lee's image of the crowd developed throughout his life.

As a youth and, later, a pastor in western New England, Lee acquired respect for his Puritan ancestors and wistfulness about their vanished authority. One of Bush's larger points, however, is that Lee differed from many exemplars of "antimodernism" (p. 32) by ascribing to Emersonian geniuses the ability to sustain ideals of character in an urban context. Believing that writing would afford him greater opportunity to play that romantic role himself, Lee in 1896 resigned his pulpit. Soon he harnessed his desire for power to an analysis of crowd behavior. For instance, his book *The Lost Art of Reading* (1902), part of a wider critical tradition than Bush indicates, blamed "mob-influence" (p. 79) for the propensity of readers to skim for facts rather than to synthesize knowledge.

Lee's *Inspired Millionaires* (1908) explicitly connected control of the crowd to the dominance of charismatic businessmen. Yet Lee continued to leave room in his scheme for the "wordsmith" capable of redeeming America by manipulating public opinion. One of Bush's strongest chapters explores (in addition to *Crowds*) Lee's embrace of three intertwined phenomena that enhanced the "wordsmith's" influence: the culture of personality, the rise of advertising, and the changing definition of news. Bush's depiction of Lee as a figure who adapted a late-nineteenth-century sensibility to mass consumer culture usefully contributes to the ongoing effort among historians to understand with more depth and precision the emergence of a modern ethos.

Throughout his account Bush is rightly critical of Lee for his condescension toward the working class. Although one would like to have seen more speculation about the sources of Lee's psychological make-up, Bush is also appropriately sensitive to Lee's megalomaniacal tendencies. At one point, he calls Lee's fantasies "unstable and absurd" (p. 126), which they decidedly were. Many of Lee's contemporaries came to the same conclusion. In the wake of *Crowds*, Lee incurred attacks from numerous intellectuals; even among business executives, his popularity plummeted in the mid-1910s. At his death in 1944, he was not the savior of the crowd but a lonely, isolated eccentric.

The sense that Lee was, at bottom, a crank makes it difficult entirely to accept Bush's claims for his subject's significance. Bush repeatedly asserts that Lee's work furnishes a "warning" about the "dangerous" consequences of misperceiving the crowd (for example, pp. 89, 172). Yet, given Lee's oddity, it is hard to see where the danger lies. (In addition, Bush's prose contains its own quirks of style and syntax.) Bush mentions that he originally intended to discuss Lee as one of a group of writers on crowds. At the risk of

committing the reviewer's ultimate sin—declaring that the author should have written a different book—one might wish that he had adopted that broader perspective rather than straining his argument to establish Lee's importance as an individual. Nonetheless, the book Bush has written will serve cultural historians well.

JOAN SHELLEY RUBIN
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ROBERT E. KOHLER. *Partners in Science: Foundations and Natural Scientists, 1900–1945*. Chicago: University of Chicago Press. 1991. Pp. xv, 415. \$34.95.

As in his previous book, *From Medical Chemistry to Biochemistry* (1982), Robert E. Kohler demonstrates his mastery of the archival materials and the contextual approach to the history of science. In the preface of this work, Kohler traces his interest in the role of foundations in science to 1974, when he wandered into the newly opened archives of the Rockefeller Foundation, shortly after his conversion from biochemistry to history. Searching the archives of foundation managers led to a vision of science as “a complex social system” that Kohler claims was “a reproach and rebuff” (p. xiv) to traditional historians of science. Kohler's goal is to achieve a history of science giving equal weight to all the social actors involved, in which the views of scientists are no more “privileged” in terms of historical value than those of “university presidents, congressmen, business, and foundation officers” (p. xiv).

Seeing science only in terms of bench research and intellectual product, Kohler contends, is too narrow a perspective; in a holistic and inclusive view of science all of the individuals involved in the enterprise of producing, justifying, funding, and organizing scientific knowledge are said to be “doing science” (p. 2). Indeed, those whom we conventionally think of as “scientists” almost invariably appear in this book in the form of “grant-getters” rather than bench workers.

The funding organizations selected for study are the seven largest foundations founded by Andrew Carnegie and John D. Rockefeller; these organizations established the system of extramural grants and provided 85 to 90 percent of foundation expenditures on science during the years between World War I and World War II. Many readers may be disappointed with Kohler's decision to focus on the natural sciences and purge his analysis of the social sciences, medicine, and public health. This decision is regrettable not only because of the vast sums spent in these fields but also because they were key areas in the early stages of the evolution of the patronage system.

Carnegie and Rockefeller initially exhibited a strong preference for supporting research institutes independent of universities. The business ideal of the

functional division of labor provided a rationale for such decisions: medical schools should teach, hospitals should care for patients, and research institutes should do research. Kohler asserts that the same argument applied to the natural sciences, but since the research “products” of the natural sciences do not necessarily fit into the same matrix of presumptive benefits as those associated with medical research, this point deserves more analysis and stronger proof.

Cutting the ties between the natural and medical sciences divisions at the Rockefeller Foundation was accomplished in the late 1930s. Programs in “experimental biology” or “vital processes” that linked biology to physics and chemistry were encouraged by Warren Weaver of the Natural Sciences Division. Weaver is portrayed as an “activist manager of science” and a protagonist in the development of “molecular biology” (p. 296). Given the new meanings attached to “molecular biology” since 1953, it is difficult to see the work of the 1930s without the distortion induced by a pronounced rosy glow. Kohler attempts to do so by making Weaver the key to the story. He does, however, briefly review the work of William Astbury, Theodor Svedberg, Dorothy Wrinch, Linus Pauling, and other researchers. Kohler concludes that Weaver's role was the promotion of the careers of scientists who “exemplified a style of transdisciplinary science” (p. 354).

The book ends at the point where “big science” first emerged and the federal government became the grand patron of science in the wake of World War II. In his brief account of this period, Kohler argues that the “pervasive presence of peer review” (p. 404) was the major difference between the old and new systems of patronage. In other words, the new bureaucratic system reduced the role and personal intrusions of program managers that were characteristic of the patronage system of the foundations.

By the 1950s the scale of federal support for the natural sciences was so great that foundation managers felt there was no longer any need for foundation support. In the 1950s and 1960s the foundations returned to the kinds of programs funded in their earliest years: health, welfare, and general education.

LOIS N. MAGNER
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ROBERT ERNST. *Weakness is a Crime: The Life of Bernarr Macfadden*. Syracuse, N.Y.: Syracuse University Press. 1991. Pp. xiv, 278. Cloth \$34.95, paper \$17.95.

Bernarr Macfadden's tombstone acclaims him the “Father of Physical Culture” (p. 206). For his early twentieth-century generation, he did indeed become the most prominent popular health adviser in America's long parade of crusaders for fitness. Equipped with only three years of formal schooling, Macfadden possessed an innate knack for showmanship. Using his own muscled body as his prime exhibit, he toured

the nation preaching the gospel of exercise, diet, and natural healing. In his magazine *Physical Culture*, Macfadden put his message for the masses into print. Later, adding sex and violence to health, he built a publishing empire, centering on *True Story*, *Liberty*, and the tabloid *New York Evening Graphic*, that reached a total annual circulation of 25 to 30 million issues. The Great Depression cost Macfadden his throne, and his political ambitions, reaching even to the presidency, made no progress.

Robert Ernst's account of Macfadden's complicated public career and tempestuous personal life is more scholarly and judicious than any of the numerous preceding biographies. The research is far-reaching and meticulous, from a perusal of thousands of Macfadden's words in print, through FBI files and the papers of presidents Franklin Roosevelt, Harry Truman, and Dwight Eisenhower, to numerous interviews with family (including the last of Macfadden's four wives) and business associates. The tale is told straightforwardly with a studied effort to be fair to a figure often maligned for his crass opportunism, mixing of personal and corporate funds, and weird health doctrines. The broad setting for Macfadden's personal adventures is well arranged.

Ernst accords Macfadden a pioneering role in journalism, his magazines being "the first to cultivate a readership among the masses" (p. 216), and among the first to use photographs to illustrate fiction. Before becoming movie stars, Norma Shearer, Fredric March, and Katharine Hepburn posed for such *True Story* pictures. Ernst also grants Macfadden complete sincerity as to his health doctrines, and while recognizing that some of his ideas were wrong, he credits Macfadden with being "ahead of his time" (p. 217) in his nutritional counsel and with helping "many" (p. 219) people with his natural healing advice. I believe Ernst is too generous in these conclusions about Macfadden's beneficial influence in the realm of health, for testimonials of gratitude, the documentary basis for claims of therapeutic utility, have no weight as scientific evidence. Indeed, many of Macfadden's ideas were dangerously extreme, like his denial of the germ theory of disease. Nearer to the mark is H. L. Mencken's severe judgment, which Ernst quotes, scorning Macfadden's "vast and cocksure ignorance" and condemning his posing as "an authority upon the crimes of modern medicine without knowing anything more about the human body than any other gymnast" (p. 211). Today's version of do-it-yourself medicine perpetuates some of the false doctrines that Macfadden adapted from his predecessors and trumpeted to the America and the England of his day.

This difference with Ernst in evaluating Macfadden's role in the nation's health is my only demur to a well-wrought and absorbing biography of a significant figure in popular culture.

JAMES HARVEY YOUNG
Emory University

SUSAN COULTRAP-MCQUIN. *Doing Literary Business: American Women Writers in the Nineteenth Century*. (Gender and American Culture.) Chapel Hill: University of North Carolina Press. 1990. Pp. xvi, 253. Cloth \$29.95, paper \$10.95.

For those fascinated by the world of American mid-nineteenth-century publishing and the way in which the careers of women writers of the period reflected changes in the publishing world's perception of itself and of authors, Susan Coultrap-McQuin's book is highly informative. With skillful, vivid, specific detail she explores the professional biographies of E. D. E. N. Southworth, Harriet Beecher Stowe, Gail Hamilton, Helen Hunt Jackson, and Elizabeth Stuart Phelps, paying special attention to their relations with their male publishers. One constant theme she develops is the way those relations ultimately mirrored the change in women writers' roles and expectations from the mid-nineteenth century through the first decade of the twentieth century. In doing so, Coultrap-McQuin also attempts, somewhat less successfully, to sketch a continuum of attitudes held by female writers toward the ideals of True Womanhood.

A complex study, this book attempts to trace two important themes simultaneously and experiences some trouble coordinating them into one coherent whole. For example, it is unclear, given the obvious contradictions to the very essence of True Womanhood, why she attempts at the end of the work to apply this ideal as fully as she does to the five professional women she chooses to study. These women obviously competed in both the literary and the business world of authorship; they also earned their own living, in most cases arguing with their publishers and negotiating for better compensation for their work. True Womanhood's ideals of submissiveness, humility, spirituality, and domesticity automatically condemn these women and their very lives. Although it is fashionable to view all shades of opinion as positions on a continuum, it is less accurate to do so with ideals that contradict each other in such basic and obvious ways. The argument that these women perhaps followed another ideal altogether seems much more likely than stating that they were simply practicing variations on an old, familiar theme.

Despite this problem, the book is full of well-researched, thoroughly documented information about the phenomenon of "gentlemen publishers" such as James T. Fields and Robert Bonner. The author explores the genteel paternalistic ideals such men held in their relations with "their" authors, and their ethical view of literature. She also traces the demise of this ideal and the rise of the proto-modern businessman publisher. She suggests that, with his arrival, the woman writer's ability to see herself simultaneously as both feminine and a professional woman lessened, as the smoke screen of a personal

relationship cleared to reveal an impersonal and definite contract on paper.

For women authors, the American publishing world of the mid-century was considerably more psychologically accommodating, permeable, and financially rewarding than was its British counterpart—and the author makes this very clear. For those who wish to understand this lost publishing era, this is valuable reading; for those attempting to identify specific female ideals, however, this work will be less illuminating.

FRANCES B. COGAN
University of Oregon

SHERILYN COX BENNION. *Equal to the Occasion: Women Editors of the Nineteenth-Century West*. (Nevada Studies in History and Political Science, number 30.) Reno: University of Nevada Press. 1990. Pp. ix, 210. \$24.95.

In this work, Sherilyn Cox Bennion provides a compendium of female newspaper editors in the Far West (including Alaska and Hawaii) from 1854 to 1900. She identifies 272 in all, listing them by name and archival repository in the appendix and providing more complete biographical information on thirty-eight of them in the course of her study. Only a handful (for example, Abigail Scott Duniway of Portland and Caroline Churchill of Denver) are well known. Bennion clearly had to dig deep to amass the amount of biographical detail she has achieved. In addition to the use of group biography, Bennion presents a typology considering in separate chapters women who were editors of western ladies' papers, small-town newspapers, reform and religious magazines, suffrage newspapers, medical journals, and literary magazines.

Some women were more daring than others. For example, the few women who were small-town newspaper editors broke into a male-dominated field; others, like the editors of suffrage papers, were public advocates of a rather unpopular political position. But most of the female editors were clustered in less-controversial areas. As in so many other fields in the late nineteenth century, there were opportunities for women as newspaper editors—if they accepted the restriction of speaking only to other women. In any case, most women did not edit newspapers for very long: 69 percent for fewer than three years. Bennion does not give us a comparable figure for western male editors. Nor does she compare the numbers of eastern and western female editors (only about 2 percent of all western editors). This is too bad, for it appears that Bennion's book might have been an interesting study of continuity between eastern and western experience. Unfortunately, Bennion begins with the Turnerian assumption that the frontier experience was uniquely different and searches to explain western women's behavior in stereotypical terms, vacillating back and forth between notions of

frontier freedom and woman's civilizing mission. Thus, although she provides good capsule descriptions of different types of newspapers and of the lives of a number of interesting women, the book makes no larger contribution to the ongoing work of western women's history. One hopes that historians of journalism, of whom Bennion is one, will find it more useful.

SUSAN ARMITAGE
Washington State University

ROBYN MUNCY. *Creating a Female Dominion in American Reform, 1890–1935*. New York: Oxford University Press. 1991. Pp. xvii, 221. \$28.00.

Historians of the Progressive Era have identified a significant female reform culture centered in the settlement houses, especially Hull House. Although many historians have stressed the defeat of Progressivism after World War I, Robyn Muncy follows Clarke Chambers in tracing the reform tradition between the Progressive Era and the New Deal. Not the failure of reform, but its survival, is her theme.

Taking as her clue the comment of one hostile observer in 1926 that "practically all the radicalism started among women in the United States centers about Hull-House, Chicago, and the Children's Bureau at Washington, with a dynasty of Hull-House graduates in charge of it" (p. 158), Muncy has identified the personal and political networks that sustained women's policy-making roles in the 1920s. Using the strikingly original metaphor of a "female dominion within the larger empire of policymaking" (p. 3), Muncy provides evidence linking women's culture and institutions in the public and private spheres, in social work, education, and government, and over several generations of reformers. Like Estelle Freedman and Margaret Rossiter, Muncy argues that women's effectiveness depended on their control of their own institutions, especially institutions for training younger women in the reform tradition. This enabled female professionals to appropriate the "space" of child and maternal health, a reform "territory" where female expertise was unchallenged. Muncy makes the metaphor work over time as well as space: the "female dominion" created by Jane Addams and Julia Lathrop in Hull House was transmitted by Edith Abbott, Grace Abbott, and Sophonisba Breckinridge through the Children's Bureau, the Chicago School of Civics and Philanthropy, and the public health field during the Sheppard-Towner period until, with the New Deal, "the sun rose on the women's agenda, but set over the dominion itself" (p. 157).

Muncy's theme that the female Progressives' influence persisted throughout the 1920s requires her to challenge the view that professionalization was hostile to reform. She takes issue with the prevailing assessment of professionalization as intrinsically coercive,

self-interested, and undemocratic, and with historians such as Regina Kunzel, Judith Trolander, and Ellen Fitzpatrick who portray professionalization as especially problematic for women. These historians have described female professionals as unable to straddle the goals of reform and social science as these two diverged in the 1920s. Muncy claims that reformers developed a "female professional ethos" that enabled them to retain their reform commitments even as they pursued professionalization. In contrast to their male counterparts, female professionals continued self-consciously to exemplify "feminine" values such as service and self-sacrifice, remained committed to both advocacy and research, worked to disseminate knowledge and not just to hoard it, and cooperated with laity through networks of *women's voluntary and reform associations*. Yet Muncy avoids essentialist explanations of how gender shaped professionalization in favor of commonsensical ones: female professionals became policy experts on children, women, and the poor because women were only listened to on these topics. The argument is historical and richly contextual, not reductive.

Unlike historians who argue that the welfare state strengthened patriarchy, Muncy admires the women who created and staffed America's "first federal program for social welfare" (p. 93), which improved health for thousands of mothers and infants. But, in focusing on the makers of social policy, Muncy only briefly considers the impact of the female dominion on the objects of reform, poor women and children. Belatedly, she concedes that these white, middle-class reformers were narrowly class-based, arrogant experts who were especially hostile to black and immigrant health providers. More attention to the impact of social policies would perhaps have tempered her enthusiasm for the social workers and reformers: it is not enough to show that women got power, we must ask how that power was used.

This complaint aside, Muncy has made an important contribution to the literature on Progressivism, feminism, and reform. Women's history, long captivated by the metaphor of separate spheres, has acquired with the "female dominion" a new way of understanding how women functioned within male-dominated society in early twentieth-century America.

RUTH HUTCHINSON CROCKER
Auburn University

ROBERT C. BANNISTER. *Jessie Bernard: The Making of a Feminist*. New Brunswick, N.J.: Rutgers University Press. 1991. Pp. xii, 276. \$27.95.

Ambivalence is a word that keeps cropping up in this trenchant account of sociologist Jessie Bernard's quest for personal and professional identity. Robert C. Bannister cogently traces Bernard's evolution from a highly positivist, quantitative sociology,

learned at the University of Minnesota and from her mentor-lover-husband Luther Bernard, to a more critical, feminist sociological perspective, a shift indebted in part to Thomas Kuhn's work, *The Structure of Scientific Revolutions* (1961), and to the emerging women's movement of the late 1960s. The author of such pioneering feminist works as *Women and the Public Interest* (1971), *The Future of Marriage* (1972), *The Future of Motherhood* (1974), and *The Female World* (1981), Jessie Bernard came to feminism in her sixties, when she did her most influential work. She offered a strong critique of male bias in the social sciences and an emphasis on gender differences in the experience of social institutions such as marriage. Her wide-ranging, accessible works helped document the pervasiveness of sexism while also formulating suggestive hypotheses about a distinctive female culture. Yet, according to Bannister, Bernard remained "ambivalent and often ambiguous concerning the shape of a feminist sociology" (p. 208). More liberal than radical, Bernard distrusted Marxism and sociological explanations based on class, and she remained unsympathetic to various versions of socialist feminism. She mediated between liberal and radical wings of the feminist movement, preferring to seek strategies for coexistence. "I mind being restricted to any one ideological position," she wrote, "My preferred stance is non- or omni-ideological" (p. 220). Bannister suggests convincingly that she may be better remembered as a generous mentor and role model than as a feminist theorist.

The most poignant chapters of this book trace Bernard's anguished relationship with Luther Bernard, the professor of sociology whom she met as a student at the University of Minnesota in the early 1920s. A staunch behaviorist and radical environmentalist, Luther was also authoritarian, misogynistic, philandering, and anti-Semitic, indefatigably insisting that his lover, who later became his wife, accede to his often cruel demands. Unable to end the marriage and ambivalent about her own Jewish identity, Jessie nonetheless negotiated significant space for herself. Her growing prominence in the profession and her desire for children, of which they had three before Luther's death in 1961, led to a continuing battle, documented painfully in the letters that Bannister uses with great sensitivity.

Yet something of Jessie Bernard's ambivalence pervades this biography as well. Bannister charts with approval her odyssey from positivism to a more critical feminist stance, noting the costs of her continuing inability to entirely cast off the scientific legacy of her past. But he stops short of asserting his own feminist point of view, ironically identifying himself with the more positivist, empiricist position whose limitations he so persuasively argues. At the same time, Bannister powerfully conveys the enormous difficulties confronting ambitious, intellectual women in academia between the first and second waves of feminism. This account of Jessie Bernard's

career stands as moving testimony to the necessity for a feminist movement, as well as witness to the life of one of its pioneering figures.

ALICE WEXLER
Riverside, California

DOROTHY ROSS. *The Origins of American Social Science*. (Ideas in Context.) New York: Cambridge University Press. 1991. Pp. xxii, 508. \$29.95.

This is a book of levels. On the topmost level Dorothy Ross details the intellectual development of three of the major social sciences (sociology, economics, and political science), tracing them from their eighteenth-century European backgrounds to 1929, by which time the form and nature of these disciplines was set for the rest of the twentieth century. She focuses on the leading figures in each discipline, particularly those from the 1870s to the 1920s, the formative period for professional American social science, and delineates the most influential theoretical approaches. This book therefore can be read for the purpose of understanding the history of these three disciplines and for the factual information it contains.

Below this informational level is another level asking questions about objectivity and theorizing on the ideological underpinnings of "objective" social science. Ross concludes that the twentieth-century development of scientific objectivity in sociology, economics, and political science occurred as a result of a crisis over the national ideology of "American exceptionalism" (p. xiv), the idea that America is unique and millennial and outside the processes of historical change. Ross sees these three social sciences forming at a time when the idea of American exceptionalism was being challenged by the effects of the Industrial Revolution. The form and theoretical foundations of these new professional disciplines, she argues, was shaped by the attempt to remold exceptionalist ideas to fit the new conditions at the end of the nineteenth century, which led them to scientism and an underlying liberal vision. Within the new scientific parameters, social science helped keep American exceptionalism alive.

A third level concerns the question of ahistoricity in twentieth-century social sciences. Ross contends that even though sociology, economics, and political science claim to have no interest in history, their twentieth-century form itself was a response to questions of and about history and that these questions in fact shaped the development and form of "objectivity." In this account, history seems to play almost a hegemonic role in the shaping of these disciplines.

Ross's ideas on the ideology of American exceptionalism and the role of history in the creation of social science objectivity and ahistoricity are innovative and original. But her decision to marginalize psychology and anthropology from her analysis as "only partially and indirectly involved in the common

discourse about American exceptionalism" (p. xx) means that her conclusions cannot be generalized to the social sciences as a whole, as the title of the book implies. Her analysis, while thorough, remains partial.

One of the most interesting chapters examines the influence of socialism on the development of social science ideas in the late nineteenth century, particularly in economics and sociology. Most studies examine the influence of Darwinism during this period so this change of perspective allows a more inclusive picture of the influences shaping the social sciences at this time.

Two other positive elements in this study are the short comparisons, scattered within chapters, with what is happening in European social science, which help one gain a wider understanding of developments. Her comments on gender also suggest interesting insights into these disciplines, such as that "Scientism helped to set the masculine boundaries of sociology against the feminine precincts of social work and reform" (p. 394).

As an intellectual history concentrating on discourses in the social sciences, this is a dense book—well written but difficult to read without taking time and effort. Whether one agrees or disagrees with her theories, however, Ross's work enlivens our own disciplinary discourse about the history of the social sciences.

MARGARET M. CAFFREY
Memphis State University

JAMES WHITESIDE. *Regulating Danger: The Struggle for Mine Safety in the Rocky Mountain Coal Industry*. Lincoln: University of Nebraska Press. 1990. Pp. xv, 265. \$37.50.

Coal mines have been, in all times and places, dangerous places to work. The bituminous mines of the western United States have been extraordinarily hazardous. In this study, James Whiteside illuminates both the risks of extractive work and the efforts to minimize these risks in Colorado, New Mexico, Wyoming, Montana, and Utah from the 1880s up to the present. Whiteside seeks not only to retrieve from obscurity the loss of more than eight thousand lives over this period but also to replace simplistic interpretations of this carnage. Contending that "the ruthless coal baron is not an adequate explanation of the causes of danger" (p. xii), he fashions a broad multifaceted analysis of this complex issue.

The study devotes considerable attention to the economic and technological ingredients in unsafe working conditions. It delineates the structure of the industry, describing the dominant roles of the railroads and the Colorado Fuel and Iron Company and the persistence of numerous tiny "wagon" mines. Larger firms could afford to take steps to control many hazards; smaller operations frequently could

not. Whiteside traces the increasing use of mechanical and power equipment from the late nineteenth century and the ambivalent impact of mechanization on safety. New methods of production left one crucial aspect of the employment relationship unchanged: miners were generally still paid by their output. Because time spent on timbering and other accident-prevention tasks went uncompensated, there was an inevitable tendency to maximize productivity and wages and to neglect safety work. The greatest strength of this book is its emphasis on this enduring, fundamental conflict.

A political solution to the problem of miners' vulnerability to injury proved elusive. Whiteside recounts recurrent legislative initiatives at the state level and growing involvement of the federal government with this matter. A long list of stalled bills and loophole-ridden statutes demonstrates not merely the political clout of the mine operators but the hesitancy of an individualistic culture to do much more than blame the victim for injuries on the job. The study also helpfully delves into the myriad difficulties of enforcing regulations, especially in Colorado.

Whiteside has produced considerable useful information on a neglected topic. He has made a real contribution to the historical literature of occupational safety, coal mining, and the West. Potential readers should be aware of some limitations, however, of depth and breadth of analysis. For example, rather than systematically comparing the injury rates from falls of roof and coal in highly mechanized mines with the rates in less-mechanized workings, the author asserts (four times) that noisy machines precluded careful testing of the stability of overhead conditions. Whiteside often fails to place his findings in a meaningful context. He discusses early victim-blaming ideology without reference to Social Darwinism. His treatment of corporate safety programs does not locate these within the national Safety First movement. He does not relate workers' compensation legislation to progressivism. The monograph focuses on traumatic injuries to the virtual exclusion of occupational illnesses. Nonetheless, it provides a useful starting point for further investigation of working conditions in the coal industry.

ALAN DERICKSON
Pennsylvania State University

ROBERT DAVID WARD and WILLIAM WARREN ROGERS. *Convicts, Coal, and the Banner Mine Tragedy*. Tuscaloosa: University of Alabama Press. 1987. Pp. x, 159. \$19.95.

Robert David Ward and William Warren Rogers have once again mined the rich seam of Alabama's coal mining history. As they did in 1965 in *Labor Revolt in Alabama: The Great Strike of 1894*, the authors' slim new study uses a single event—the April 8, 1911, Banner mine explosion—as a prism through which to

see larger political and economic developments in Alabama during a particular era. The explosion killed 128 convict miners: 90 percent were African Americans convicted of minor crimes such as liquor selling, vagrancy, and public drunkenness who had been given what amounted to indeterminate sentences digging coal at the Pratt Consolidated mine near Birmingham.

Ward and Rogers's accomplishments are modest. The book's seven short chapters offer a sketchy narrative of the day of the explosion and press speculations about its causes; a capsule history of the convict lease system, which began in Alabama in 1839, and the failed efforts to reform it in the nineteenth century; a brief discussion of the state's formal investigation, which exonerated Pratt Consolidated officials and blamed the explosion's victims, and a subsequent federal investigation, which concluded that unsafe methane gas levels caused the tragedy; accounts of state legislative debates in 1911 on the fate of two reform bills, one of which resulted in the passage of a modest mine safety law ten days after the explosion, the other a failed bill that would have abolished the convict labor system; and a short conclusion that traces half-hearted state legislative efforts in the years after 1911 that finally ended this despicable system of forced labor in 1928, ninety years after it began.

Ward and Rogers demonstrate how the sheer, unadulterated greed of the state's industrial and political leaders kept the system alive. It was, by far, the most profitable convict lease system in the country, netting \$300,000 for Alabama's coffers in the two years after the explosion. This fact alone helps explain why Alabama clung so tenaciously to its convict labor system long after Georgia, Louisiana, and Mississippi abolished theirs. Rogers and Ward also reminds us that Progressive-era industrial reform was at best partial and regional. Unlike New York State Progressives, who mobilized successfully in the aftermath of the Triangle Shirtwaist Fire (which occurred just two weeks before the Banner explosion), Alabama's narrow reform coalition simply could not overcome the argument from business that convict labor was the only barrier that stood between industrial peace and prosperity and unending strikes by organized labor. The authors conclude, quite correctly, that convict leasing served as a kind of Black Code for the New South, assuring a cheap and docile African-American labor force for the state's growing industrial economy.

This book, however, lacks any sense of how the workingmen trapped in this heinous system experienced their condition. Ward and Rogers tell us very little about the convicts themselves: who they were, what they thought, what they experienced, how they acted. The authors acknowledge the difficulty in telling "the full story of the convicts themselves" in large part because they claim that "biographical information on them is almost nonexistent" (p. 5). But

it is hard to imagine that a system that lasted until 1928 would not yield at least some survivors, however superannuated, who could be interviewed. There are also cultural artifacts of forced labor—such as work and prison songs—that could be used to good effect to help recount the prisoners' story. Unless the tragic history of convict leasing is told from the workers' perspective, it remains at best a partial one.

STEPHEN BRIER
American Social History Project
Hunter College,
City University of New York

JOE WILLIAM TROTTER, JR. *Coal, Class, and Color: Blacks in Southern West Virginia, 1915–32*. (Blacks in the New World; The Working Class in American History.) Urbana: University of Illinois Press. 1990. Pp. xvi, 290. Cloth \$44.95, paper \$14.95.

Joe William Trotter, Jr., provides a detailed examination of the process by which southern rural and semirural blacks became a coal-mining proletariat in southern West Virginia. Although recent scholarly attention has focused on violent labor-management conflicts and the tradition of interracial unionism, Trotter's contribution is to carefully delineate the changes in the cultural life of the growing black communities of the region resulting from the expansion of the black male coal-mining proletariat.

Although coal operators rendered equal pay for equal work, they practiced racial discrimination in the allocation of jobs, in the operation of company towns, and in the administration of the state. Blacks were more likely to get the most hazardous jobs and were less likely to obtain skilled and supervisory positions. Rooming houses, cafes, poolrooms, theaters, schools, and social welfare institutions were all racially segregated.

Given this segregation, the growth of the black mining work force led to the expansion of a black middle class and elite of business people and professionals that catered to the black working-class market. The development of the black working class also led to a corresponding growth in religious, fraternal, political, and civil rights organizations. Although elite men usually assumed the major leadership roles in these organizations, miners and women also participated in leadership and put their distinctive stamps on many of these community institutions. For example, miners tended to gather in Baptist churches that continued to offer an emotional church service similar to that of black southern churches despite the urging of the black elite for a more educated ministry and a more rational religious style.

Trotter emphasizes that the retention of the franchise by West Virginia blacks allowed the expanding black community to elect blacks to local and state offices and to make substantial gains in education and social welfare programs, if on a segregated basis.

Under the leadership of the NAACP, moreover, blacks secured the passage of a state antilynching law and a state law against the showing of racially inflammatory films such as *The Birth of a Nation*.

Trotter adds to our knowledge by his skillful demonstration that an alliance of black miners and the black elite produced many achievements. He is unpersuasive, however, when he downplays conflicts between the two groups. Positive comments about the elite by former miners whom Trotter interviewed in 1983 cannot reasonably be used to outweigh the contemporary evidence that miners voiced "extreme suspicion" (p. 166) of the elite when they were interviewed by James T. Laing in 1933. Trotter's characterization of the black elite role in strikebreaking as one designed to promote black job security and an indication of a growing black worker–black elite alliance is equally unsatisfying. Most black miners in the area supported the union and its defeat was a blow to their job security.

Trotter's equating of white coal operator–black elite and white miner–black miner alliances is problematic. The black elite came to terms with or established alliances with white coal operators and white Republican leaders because they were members of the regional power elite from whom the community needed to gain concessions. The alliance of black and white miners, by contrast, was an effort (unsuccessful in this period) to achieve a common egalitarian goal: a stable and recognized union that would give miners some independent working-class power in the region. If white miners were less than egalitarian toward black miners (and Trotter is unconvincing on this score), their relationship lacks the dominant/subordinate character of the relationship between white and black elites.

Despite its shortcomings, Trotter's work is notably successful in its delineation of the actual trajectory of black working-class development during a time of segregation.

MARTIN HALPERN
Henderson State University

WILLARD B. GATEWOOD. *Aristocrats of Color: The Black Elite, 1880–1920*. Bloomington: Indiana University Press. 1990. Pp. xii, 450. \$39.95.

In this book, Willard B. Gatewood aims "to identify the black elite that was predominant in the forty years following the end of Reconstruction, to explore its self-image, behavior, values, strategies, and relationship to the larger society, both white and black, and to indicate changes that occurred in its composition" (p. x). Four sections, "Origins," "People and Places," "Color, Culture, and Behavior," and "Changes and Continuities" form the broad outline of the book and include chapters that identify the elite and explore, among other things, club life, education, and Jim Crow in their lives. Gatewood describes the aristocrats

generally as free-born and/or descended from whites, educated, well-traveled, well-mannered, and having high-status positions in the black community. This volume provides much detail concerning the lives of these "aristocrats" but leaves much room for further research.

One shortcoming is Gatewood's reliance on data derived almost exclusively from secondary works and uncritiqued period publications. For example, he quotes a nineteenth-century white observer's characterization of John F. Cook as "a leader of the aristocracy among the darkies" (p. 4) without commentary. Many of the white observers noticed the black elite with shock, amusement, or both, and regularly mocked them in print. Those remarks and the observations of black commentators, too, deserve closer scrutiny.

Additionally, the failure to adequately use available manuscript materials results in an imbalanced and sometimes unnecessarily incomplete presentation. For example, a systematic examination of manuscript materials would have revealed that when Mary Church Terrell and others of "her group" used the expression, "our group," they sometimes meant not only people with similar class accoutrements but also those who possessed similar political or racial consciousness. Later, Gatewood writes that Archibald Grimké "apparently reared" his daughter, Angelina (p. 178). The papers of Charlotte (Forten), Angelina, Archibald, and Francis Grimké demonstrate conclusively that Archibald indeed reared Angelina, with considerable assistance from his brother and sister-in-law. The papers of these and other families discussed in this volume provide rich detail about their lifestyles, values, and self-images—the kind of detail that would enrich any study of such a group as this. And, considering that these "aristocrats" were the ones who left the most letters, diaries, speeches, and other writings, the portrayal we have here represents only a beginning.

Gatewood's use (or misuse) of the language of the past further blurs his portrait of this important group. It leads him in one instance to refer to the female elite as "parlor ladies," but then to note that some were "teachers, lecturers, or writers" (p. 191). Later, he adds that they were also realtors, newspaper publishers, and managers (p. 241). More important than the obvious contradiction, referring to the women as "parlor ladies" at all diminishes (and dishonors) the extensive work—paid and non-paid—that they regularly undertook. Mamie Garvin Fields depicted "parlor society ladies" in *Lemon Swamp and Other Places* (1980) as activists who met in each other's homes for benevolent and beneficial purposes. Because some of Gatewood's discussion supports Fields's more appropriate characterization, his failure to use both expressions, if necessary, and to distinguish clearly between the two, obscures the women's identities.

In many ways, the black elite in this book seem to be

ridiculed, ironically, for being what Gatewood acknowledges that they were: aristocrats of color. A historic feature of "aristocrats" is precisely the manner in which they set themselves apart from the rest of society. But their setting themselves apart socially and physically from "'the vicious, ignorant, and unreliable'" (p. 189) did not preclude, as Gatewood's data suggest, important interclass alliances, which Julie Winch demonstrated in *Philadelphia's Black Elite* (1988).

Some scholars will benefit from Gatewood's identification of so many "aristocrats of color." But many of the details concerning their values, strategies, behavior, self-image, and relationships remain to be established.

STEPHANIE J. SHAW
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Columbus

DENNIS B. DOWNEY and RAYMOND M. HYSER. *No Crooked Death: Coatesville, Pennsylvania, and the Lynching of Zachariah Walker*. (Blacks in the New World.) Urbana: University of Illinois Press. 1991. Pp. xv, 174. \$28.95.

In the United States before 1880 the "lynch law" usually connoted "frontier vigilantism" directed against horse thieves and other outlaws. This was normally a public act, committed by individuals or a mob, and, while it involved beatings and whippings, rarely did it lead to death. After 1880, however, lynching assumed a new meaning. It now referred to either an extralegal hanging or burning to death of an accused person (usually a black male), and generally involved torture and mutilation.

In 1911 alone there were seventy lynchings in the United States. One took place on August 15, in the southeastern corner of Pennsylvania. The victim, an African American named Zachariah Walker who was a recent migrant to the area, became involved in an altercation with a company policeman. In the incident (to which there were no witnesses) the policeman was killed. Walker, who had been carrying a concealed weapon (not that unusual, however, for this steel town, particularly on weekends), tried to escape the posse sent after him by hiding in a tree. After being spotted, he attempted suicide by shooting himself in the head. Seriously wounded, he fell from the tree and was taken to the local hospital. A gathering mob, upset by what they had heard about the incident and supposedly aroused by a local politician, decided to lynch Walker. With the local police providing little or no protection, the mob seized Walker, some shouting "Burn him, burn him," others kicking him or hitting him with stones and sticks. Then Walker was dragged to a field outside of Coatesville, where the mob had already built a fire. As they were about to throw him onto it, Walker cried out in appeal, "For God's sake, give a man a chance! I killed him in self-defense.

Don't give me no crooked death because I'm not white!" Unheeding, the crowd threw him onto the flames. Three times he attempted to escape, even when his body was already half-consumed by fire. But each time the mob forced him back with fence posts (p. 35).

Approximately 5,000 white people—men, women, and children—watched this atrocity, with not one person voicing an objection. In fact, after Walker's burned body cooled, it was cut up for souvenirs of the event. Despite ensuing efforts by the NAACP and the governor's office to find and try the instigators, no one was ever convicted of this crime.

Why did a lynching take place in Coatesville? Dennis B. Downey and Raymond M. Hyser assert that this "lynching did occur in a social milieu quite different from that of most, if not all, other lynchings during the era," although they do not adduce demonstrable evidence for this assertion (p. 127). Drawing on their reading of Kai Erickson and Emile Durkheim, the authors contend "that the lynching in Coatesville resulted from the residents' perceived threat to their way of life . . . by the influx of migrants (black and white southerners, and Europeans) into the community. By burning him, the participants denied Walker his very personhood" (p. 146).

The authors' attempt to find in the lynching of Walker a unique occurrence simply does not work. The social conditions they describe in Coatesville were being replicated around the country, North and South. This period was in fact rife with incidents of lynching and race riots: between 1882 and 1919, 3,000 lynchings occurred. Official indifference, if not outright encouragement, no doubt helped to goad these atrocities. And, while it was more unusual for a lynching to take place in the North, any black persons who stepped beyond their perceived "place" could end up like Walker during this period.

Each lynching in and of itself is an incident worthy of study. There is really no need to find something "exceptional" to make the story worth telling. As Leon Litwack has argued, "to count the lynchings and burnings, to detail the savagery, the methodical torture prolonged for the benefit of the spectators, to dwell on the voyeuristic sadism that characterized these ritual murders and blood rites . . . is to underscore the degree to which many whites by the late nineteenth century had come to think of black men and women as less than human" ("Trouble in Mind: The Bicentennial and the Afro-American Experience," *Journal of American History* 74 [1987], 326). While Litwack was directing his attention to the South, Downey and Hyser might have profitably explored the same idea for the North and posed these questions: What had happened to cause northerners, mostly native-born whites, male and female, to think of burning human beings alive as an acceptable way to achieve "justice"? How were these ideas transmitted to this area, or did they in fact originate there? And why did these brutal notions find a home in a

corner of Pennsylvania that had had little experience with black people? Once we can answer these questions, we will be much closer to understanding the intersection of race and violence in the United States in the late nineteenth and early twentieth centuries.

The authors titled their summary chapter "An American Tragedy." The "tragedy" of the Walker lynching lay, however, not in its atypicality but in the fact that it was indeed so typical, so American. The question still remains, why?

KENNETH W. GOINGS

Florida Atlantic University

PAUL AVRICH. *Sacco and Vanzetti: The Anarchist Background*. Princeton: Princeton University Press. 1991. Pp. x. 265. \$24.95.

Why yet another book about Sacco and Vanzetti? As Paul Avrich, the premier historian of American anarchism, notes in his introduction, "No other case of the period attracted more widespread attention, for it had a significance that made it symbolic of its time and place, elevating it to the level of tragedy and uncovering an aspect of American society that would not otherwise have been so nakedly exposed" (pp. 4–5). But most of the scholarly and popular attention has focused either on the crime for which these two Italian anarchists were convicted or the celebrated trial that led to their execution. Avrich instead sets his sights on the political movement to which they dedicated their lives. A small mountain of literature has arisen from the case, but no one has taken this perspective or examined all of the relevant Italian language sources. The result is a book that provides a much better context for understanding the world of Sacco and Vanzetti.

Avrich is inclined to think that the two were innocent of the robbery and murder charges for which they were executed in 1927 but guilty of being ardent followers of Luigi Galleani's group that was at the center of the violent anarchist politics of the 1919–21 era. He describes the men's rural Italian backgrounds and their early years in the United States, but much of the book is devoted to the adventures and misadventures of various anarchist bombers and their brushes with federal authorities. The narrative often strays rather far from Sacco and Vanzetti's own activities, and Avrich never quite implicates them directly in a bombing. But this was clearly their element, and he does a remarkable job of following the activities of this subterranean movement through a wide range of sources.

For all of his detective work, there is one mystery, perhaps the biggest one, that Avrich does not fully solve. If he is correct about Sacco and Vanzetti's involvement with the bombings, then what was it about their situation and that of some other Italian immigrants that turned them to terrorism? The background he provides on each of the men, and much of

what else we know of them, suggests two very gentle people, typical in many respects of other immigrant workers. Indeed, their typicality encouraged millions of immigrants from quite diverse political perspectives to identify with them, and their perceived victimization touched off a worldwide protest movement. What moved them beyond their more common pursuits of strike support, writing, and speaking to take up dynamite? The answer lies beyond the fascinating world of the anarchists, in the daily reality of the immigrant worker and in the vision of a more just society that Sacco and Vanzetti shared with many others inside and outside that movement.

JAMES R. BARRETT
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RICHARD W. JUDD. *Socialist Cities: Municipal Politics and the Grass Roots of American Socialism*. (SUNY Series in American Labor History.) Albany: State University of New York Press. 1989. Pp. x, 254. Cloth \$54.50, paper \$18.95.

The rise and fall of American socialism during the early twentieth century has generated substantial interest and controversy among historians. Central to the debate have been opposing theses of the party's rapid rise followed by its sudden decline. Early histories of the party, written for the most part during the 1950s and 1960s, generally concurred that the party's initial success was the result of its moderate reform program that appealed to middle-class voters disaffected with the corruption and inaction of Republicans and Democrats. For some, however, the party lost popular support once it replaced its pragmatic reform program and rejected capitalism, consequently placing itself outside of the country's liberal tradition. For still others the party foundered precisely because it diluted its revolutionary program to become the left wing of the Progressive movement. Once traditional parties embraced Progressivism their reform program became indistinguishable from that of the Socialist Party and therefore permanently undermined its appeal. Finally, a third interpretation attributed the party's demise to the antiradical persecution incited by World War I and to internal divisions that followed the Bolshevik Revolution.

Notwithstanding their differences, all these works have focused on the party's national leadership. Recent studies have moved away from a traditional political history toward a more elaborate social history of the movement. They have concentrated on socialist activities in ethnic and racial communities, toward farmers, among women, and within the labor movement. Several works have dealt with socialist activities at the municipal level, where the party achieved its most important political successes. Socialists believed that city politics represented a natural arena from which they could spread their ideas, broaden their

base of support, and that would ultimately serve as a basis for future political achievements.

Richard W. Judd presents case studies of six cities administered by Socialists in the heart of the industrial Midwest. He convincingly shows that the party's rise and fall in the cities was determined by the shifting composition of its social base of electoral support. Socialists were voted into office by the convergence of class-conscious industrial workers with middle-class reformers reacting to corruption within traditional parties. Socialists, however, were unable to transform electoral victories into permanent political realignments. They promoted social and structural reforms that appealed to workers, but they were vehemently opposed by local business and political interests and middle-class reformers who wanted government to be honest and efficient but not more democratic. Consequently, Socialist victories unified otherwise divided forces and led to a new political realignment along class lines. At subsequent elections Socialists maintained and expanded their working-class constituency but were defeated by fusion tickets running on reform-oriented Progressive programs. By focusing on the interaction between Socialist policies at the grass roots and the class composition of their supporters and their opponents, Judd provides fresh insight into the character of American Socialism as well as the political and economic interests behind the development of the Progressive movement.

FRASER OTTANELLI
University of South Florida

ROBIN D. G. KELLEY. *Hammer and Hoe: Alabama Communists during the Great Depression*. (The Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1990. Pp. xxiii, 369. Cloth \$34.95, paper \$12.95.

At the outset of the Great Depression, organizers for the Communist Party arrived in Birmingham, Alabama, the industrial center of the Deep South whose industrialists and politicians had repeatedly crushed an interracial movement of mine workers and vigilantly enforced a system based on white supremacy. As Robin D. G. Kelley shows in his social and cultural history, party members over the course of the 1930s led marches of the unemployed, resisted evictions and foreclosures, organized sharecroppers, advocated dual unionism, then formed rank-and-file committees within existing unions and, ultimately and enthusiastically, embraced the Congress of Industrial Organizations (CIO). They also published newspapers, protested police brutality and the denial of civil liberties, denounced and publicized lynchings and blatant miscarriages of justice (such as the Scottsboro case, as well as its many less well known counterparts), alternately criticized and sought alliances with the black elite, conducted right-to-vote campaigns, and participated in a wide range of political coalitions. If

their numbers were generally small—ranging from a few dozen to many thousand, sympathizers included—their impact was not, Kelley contends. Playing the gadfly, Alabama Communists raised critical issues and prompted some existing organizations to adopt stronger measures on issues of black equality and social justice.

The party's interest in southern blacks derived in part from its theorists' analysis of the region, their contention that southern blacks constituted an oppressed nation, and their program of self-determination for the Black Belt. In contrast, African Americans' interest in the party often had less programmatic and more practical roots. In Alabama, the party was the one voice that unequivocally and aggressively stood for black equality. Whereas some local black elites might espouse conservative values—including a virulent anti-unionism and a Booker T. Washingtonesque emphasis on hard work and thrift—and others might promote quieter, selective legal challenges to the most blatant of abuses (as did the Birmingham branch of the NAACP), the party was nothing if not vocal and militant. Itself a "race" organization, the party became a "working-class alternative to the NAACP" (p. xii).

Kelley provides a detailed social profile of the party's rank and file. During the first half of the decade, the party's base consisted primarily of unskilled and semiskilled black workers who labored in the region's steel mills, coal and iron mines, and, for female domestic servants, in the homes of Birmingham's white families. Many came from rural backgrounds, were poor, illiterate, and often religious. These men and women, Kelley argues, built Alabama's largely decentralized party "from scratch." Possessing no "Euro-American left wing tradition" (p. 93), they instead espoused a "homegrown radicalism" born of their desire to challenge racial domination (p. xii). Alabama's black Communists were never blank sheets on which white Communists could impose their views; rather, they interpreted Marxism from their distinct historical perspectives, drawing on a long and "rich culture of opposition" (p. 93). When party leaders shifted their emphasis away from black issues and toward establishing the party's legitimacy by building bridges to southern liberals during the period of the Popular and Democrat fronts of the late 1930s, many black workers responded by shifting their energies in other directions (particularly toward the CIO). It was the party's stance on local, immediate issues that served initially to attract blacks to its ranks; later in the decade, the party's new stances on local issues prompted blacks to lose interest as well. In Kelley's analysis, abrupt alterations in the party's foreign policy played little role in black workers' decision making.

As Kelley makes clear in numerous powerful accounts, to be a Communist in depression-era Alabama required, above all else, the wherewithal to withstand almost unrelenting harassment and repres-

sion. Communists faced beatings, kidnappings, and lynchings at the hands of vigilante mobs, while their demonstrations and marches provoked violent attacks. Congressional investigations led by congressmen Hamilton Fish and Martin Dies aired false and inflammatory charges against the organization, and police routinely harassed and arrested local party members on charges of vagrancy, "criminal anarchy," and advocating "social equality" between blacks and whites. If the intensity of repression varied over time, the threat of violent reprisals was a constant. In a region known for its hostility to African-American equality and organized labor, Alabama's Communist Party suffered the added burden of a fierce, deeply rooted anticommunism.

This book fills an important gap in the growing historical literature on American communism, complementing the narratives of Hosea Hudson and Nate Shaw, and Mark Naison's study of the party in Harlem. As a work that bridges the fields of African-American, labor, and cultural history, Kelley's book raises important questions about the relationship between black rank-and-file activists and social protest movements, demonstrating the inadequacy of older interpretive models. Alabama's white supremacists and black moderates may have accused the Communists of using black workers in their struggles against capitalism; Kelley shows us how black workers themselves used the party to further their own ends of racial and economic equality.

ERIC ARNESEN
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FRASER M. OTTANELLI. *The Communist Party of the United States: From the Depression to World War II*. New Brunswick, N.J.: Rutgers University Press. 1991. Pp. x, 307. Cloth \$39.00, paper \$14.00.

Fraser M. Ottanelli has set out to revise the historiography of American communism by writing a synthesis that emphasizes the interaction between grass-roots activity and national leadership, a relationship he claims historians have ignored. His basic argument is that the policy of the Communist Party of the United States of America (CPUSA) was not made in Moscow but initiated by U.S. Communists responding to local conditions. The party gained popularity as a result of its own efforts at "Americanizing" its policies, language, and organizational forms. Comintern "directives" for the most part coincided with the push of U.S. Communist leaders to escape the sectarianism of Third Period communism and build alliances with progressives outside of the party.

Anyone conversant with CPUSA historiography will recognize versions of this argument in the works of Maurice Isserman, Paul Buhle, and Mark Naison, among others. Ottanelli's work diverges from these studies in that, except for the first half of chapter 2, he focuses almost exclusively on top-level decision

making. Although he claims to utilize "the new insights of social historians together with published and oral recollections of activists as a vantage point from which to take a fresh look and reinterpret institutional sources" (p. 4), most of his primary evidence relies on a surprisingly scant body of published party sources and speeches by national leaders, notably Earl Browder. Indeed, he not only ignores several important archival and oral history collections but also some very critical CPUSA publications such as the *Liberator*, *Negro Worker*, *Working Woman/Woman Today*, and the *Young Worker*.

The book's weaknesses are not simply a problem of sources. In his discussion of the period from the Popular Front to the end of World War II, he emphasizes the dialogue between U.S. Communist leaders and Comintern officials and all but ignores local and regional differences and conflicts. Although he has made an admirable attempt to place more weight on international factors and institutional concerns, he has skewed a complicated story that the new social history has sought to unravel. Local Communists and members of mass organizations were not of one mind on issues of policy, and in many cases national CPUSA leaders misinterpreted local conditions without any help from Moscow. Furthermore, Ottanelli says practically nothing about women or people of color, and what he does say about African Americans is both cursory and, in some instances, mistaken. His claim that the CPUSA only became interested in African Americans after the Sixth World Congress in 1928 essentially ignores the decisions of the Fourth World Congress in 1922, the influence of the African Blood Brotherhood, and the formation of the American Negro Labor Congress—efforts that were to a large degree initiated by African-American radicals themselves. Moreover, he supports his assertion that the self-determination thesis was meaningless to blacks by quoting Earl Browder and William Weinstone instead of black Communists.

If he had extended his research to include more grass-roots voices and incorporated a more nuanced reading of the new social history, he would have produced a much stronger, richer synthesis of the CPUSA during its heyday. Nevertheless, given the enormously difficult task he set out to accomplish, Ottanelli has done an admirable job and produced a valuable contribution to the field.

ROBIN D. G. KELLEY
University of Michigan,
Ann Arbor

CASEY NELSON BLAKE. *Beloved Community: The Cultural Criticism of Randolph Bourne, Van Wyck Brooks, Waldo Frank, and Lewis Mumford*. (Cultural Studies of the United States.) Chapel Hill: University of North Carolina Press. 1990. Pp. xvi, 365. Cloth \$34.95, paper \$12.95.

The "Young Americans," Randolph Bourne, Van Wyck Brooks, Waldo Frank, and Lewis Mumford, have been difficult for historians to categorize and to evaluate. Central figures in New York's intellectual life from the 1910s through World War II, except for Bourne who died in 1919, their work collectively embodied the cultural transition from Victorianism to modernism. Despite their close association, each offered somewhat different prescriptions for the modern dilemma and, over several decades, each adopted a variety of positions so that a single, coherent viewpoint never developed. Casey Nelson Blake, to his credit, has presented the Young Americans as complex and inconsistent. Written in clear, largely jargon-free prose, his book will be the standard study of these four interwar intellectuals.

The "critical project" of the Young Americans, according to Blake, in one of his few lapses into the jargon of contemporary literary theory, was to create a "beloved community" consisting of public-spirited, intellectually engaged individuals. The Young Americans sought to replace their mothers' spiritually empty Victorian gentility and their fathers' materialistically driven corporate capitalism with a modern culture that centered on aesthetic concerns and embraced individuality, sexual equality, creativity, and scientific knowledge. Stung by John Dewey's support of American intervention into World War I, the Young Americans abandoned political reform in favor of a cultural renaissance. Blake suggests that the Young Americans' withdrawal from politics and their estrangement from Dewey after 1917 seriously weakened their thought. Despite disagreement over American intervention, Dewey and the Young Americans shared a common goal of a more humane, personally fulfilling, democratic society. Blake believes that had they engaged each other in a constructive dialogue after World War I instead of polemical invective, Dewey's insistence on the primacy of politics and his unabashed democratic commitments would have saved the Young Americans from their politically naive aestheticism and cultural elitism. For their part, they would have tempered Dewey's overly scientific instrumentalism with their greater sensitivity to subjective consciousness. Thus, despite Blake's evident sympathy for the Young Americans, he aligns himself with current efforts to restore Dewey's reputation as a radical thinker, especially his ideas on education and aesthetics.

On virtually all counts Blake succeeds. He offers a convincing account of Bourne, Brooks, Frank, and Mumford and, in the process, establishes their vitality and integrity as cultural critics. They were neither chauvinistic cultural nationalists, as some have described them, nor the exponents of an emerging consumer culture of self-indulgent hedonism, as others have argued. Rather, they undertook the serious enterprise of reformulating American culture, addressing the specifics of American experience, acknowledging its immense diversity, and affirming the

value of individual creativity and freedom. Of course, they failed. Twentieth-century America did not become a "beloved community." Still, according to Blake, they contributed importantly to the continuing efforts to create a personally meaningful and socially responsible modern culture.

If anything, Blake makes too much of too little. Although interesting and important, the legacy of the Young Americans, finally, is confusing and, at times, disturbing. Even Blake's hero, Mumford, occasionally flirted with technocratic authoritarianism and, after World War II, succumbed to isolated despair. Blake is correct, however, in insisting that the Young Americans provide important insights into the critical generational/cultural shift that occurred among educated Americans in the first half of the twentieth century and, especially, for understanding the pivotal decade of the 1920s, often inaccurately labeled as a "lost generation." Far from it. In the 1920s, a rich and creative time, as Blake makes clear, American writers, artists, and intellectuals laid the foundation of modern American culture. Well written and reasoned, Blake's book is an important chapter of that story and a key book for an understanding of twentieth-century American culture.

WILLIAM B. SCOTT
Kenyon College

NEIL JUMONVILLE. *Critical Crossings: The New York Intellectuals in Postwar America*. Berkeley and Los Angeles: University of California Press. 1991. Pp. xix, 291. \$24.95.

Neil Jumonville has written a strong and enjoyable book. Rather than re-create debates over abstract ideas such as socialism, anticommunism, or American culture in general, he organizes his chapters around the responses of the New York intellectuals in their forums (the American Committee for Cultural Freedom, for example), and magazines (prominently *Partisan Review*, *Politics*, *Dissent*, *Commentary*, and *Encounter*) to a series of specific events during the years 1945–70: the Waldorf Conference of 1949; the confessional literature produced by repentant ex-Communists (such as Whittaker Chambers' *Witness*); mass culture as represented by radio, television, Hollywood movies, mass-market books and magazines, advertising, mass-produced goods and art; the Beats; and the New Left and the counterculture of the 1960s.

All New York intellectuals did not think alike about these events, people, and topics. Although many in the group had in common growing up in poorer Jewish neighborhoods and attending the City College of New York, there were generational differences. Jumonville's basic distinction involves those whom he labels "affirmers" and "dissenters." Enacting the "critical crossing" from an "earlier ideological faith and prophetic partisanship" and adopting a "more mod-

est and precise outlook based on reason, analysis, and pragmatism" (p. xii), the group's leader, Sidney Hook, and other "affirmers" such as Lionel Trilling, Clement Greenberg, Norman Podhoretz, and Nathan Glazer, usually had moved from the left before the war to the liberal center in the late 1940s and 1950s and then to the right of center in the late 1960s. They urged that, in time of cold war, the task of intellectuals was to be positive about American culture and politics. "Dissenters" such as Dwight Macdonald, Irving Howe, Lewis Coser, and Alfred Kazin, having battled Stalinists in the 1930s, shared the firm anti-communist outlook of the affirmers but moved only from the radical Left to the liberal Left after the war. Affirmers and dissenters agreed on what it meant to be an intellectual: "a nonconformist who maintained some tension with society." They did not agree on "what constituted nonconformism and about the proper level of tension with one's culture" (p. 99).

Many New York intellectuals did not, as one familiar version of their story goes, abandon a commitment to socialism in the fires of the 1960s and, in fear and loathing of the New Left, become the *Commentary* crew of the 1970s and 1980s. Emerging during the 1930s were the characteristics of what became "neo-conservatism": strong anticommunism, a reluctance to tolerate criticism of America, a fear of direct democracy, a Madisonian ideal of representative democracy rather than popular rule, a hostile and elitist attitude toward mass culture, and a "tragic view of life" (pp. 237–38, n. 4). Scorning what they perceived to be the "irrationalism . . . nihilism . . . romanticism . . . moralism, absolutism, and inflexibility" of student radicals in the 1960s, they echoed a quarter-century's commitment to "rationalism, pluralism, system, temperance, moderation, informed debate, analysis, civility, pragmatism, and reason" (p. 221).

Jumonville has not imposed on his intellectuals an agenda derived from concerns of other critics in the 1960s. Instead, he gives us the New York intellectuals in their own terms and asks us to consider questions raised by these activists, questions that resonate in today's debates over "cultural literacy," "diversity," and "closed minds" in American education. He criticizes his subjects gently and offers them as worthy "generalists" who accepted a public responsibility to promote their vision of the good society. Readers of this fine book may not sympathize with his subjects to the extent that Jumonville often does, but they will probably agree that he makes a convincing case for rejecting the commonplace of the past two decades, "that intellectual history is elitist and social history is radical" (p. xv).

ELEN NORE
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FRANK DONNER. *Protectors of Privilege: Red Squads and Police Repression in Urban America*. Berkeley and Los

Angeles: University of California Press. 1990. Pp. xiv, 503. \$34.95.

We Americans find it easy to distinguish ourselves from those countries we call "police states," and yet in every major American city the police carry on surveillance and harassment of individuals and groups that they consider "subversive" but which in other countries we would view admiringly as "dissidents."

Frank Donner is a long-time investigator of violations of civil liberties in this country. His previous books, *The Un-Americans* (1961) and *The Age of Surveillance* (1980), made important contributions to our knowledge of how congressional committees and federal agencies violated the First Amendment and Fourth Amendment rights of citizens. Now he has turned his prodigious research talents to the police forces of our cities, tracing their activities from the end of the Civil War into the 1980s, concentrating on Chicago, New York, Philadelphia, Los Angeles, and five other urban centers.

His subtitle is "Red Squads and Police Repression in Urban America," but had I been his editor I would have eliminated the term "red squads" as misleading. The search for "Communists" has been only a small part of the work of these special police units. Their net has been cast much wider, intended to trap, injure, at times kill, and always to intimidate those persons protesting against war or racial discrimination or economic injustice—whether black militants, radicals, pacifists, civil libertarians, or just activist reformers.

About one-fourth of the book is a historical survey, starting with the use of police to suppress strikers after the Civil War. Rapid industrialization brought a Gilded Age for millionaires but horrible conditions for native and foreign-born workers. The Tompkins Square Riot in Manhattan in 1874 and the execution of four Chicago anarchists in 1887 were part of a complex history of labor struggle in which police often killed striking workers. In the Progressive Era, Socialists and Wobblies were the target of police attacks, and the anarchist-feminist Emma Goldman was arrested over a hundred times by police in various cities simply for making speeches that the rich and powerful could not tolerate.

In the 1930s there were real Communists to spy on, but the "red squads" were most often used against strikers in the great labor uprisings of that time. In the Memorial Day Massacre of 1937, police fired at the backs of demonstrators during a Republic Steel strike and killed ten people. During the 1960s southern police chiefs conspired with the FBI to attack Freedom Riders, and all over the country police infiltrated groups opposed to the Vietnam War.

The main part of the book consists of detailed studies of police assaults on civil liberties in Chicago, New York, Los Angeles, Philadelphia, and briefer looks at Detroit, Baltimore, Birmingham, New Haven, and Washington, D.C. Donner observes that

obsessions with radicalism and conspiracy, and lately with "terrorism," have been ways of diverting attention from the real problems of racism and economic injustice.

An epilogue takes us close to the present and gives little comfort that these practices will stop. Indeed, there is bound to be a continuing struggle between police surveillance and citizen resistance until the solution of fundamental problems lessens both the need for protest movements and the inevitable efforts of the establishment to intimidate these movements.

HOWARD ZINN

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JON C. TEAFORD. *The Rough Road to Renaissance: Urban Revitalization in America, 1940–1985*. (Creating the North American Landscape.) Baltimore: Johns Hopkins University Press. 1990. Pp. x, 383. Cloth \$48.50, paper \$16.95.

The publication of Jon C. Teaford's *The Unheralded Triumph* (1984) challenged textbook authors to rethink and sometimes rewrite their chapters on the governance of late-nineteenth-century American cities. This latest handiwork by Teaford, although valuable in many respects, is unlikely to cause a similar reappraisal by historians of late-twentieth-century urban America. If *The Unheralded Triumph* drew so much attention because of its assault on the elaborate conceptual scaffolding that has structured scholarly study of the industrial city, Teaford's new book falls into potholes because of the absence of such an apparatus to analyze the postindustrial city. We have not formed a good grip on the modern metropolis and, despite Teaford's herculean efforts, we still do not have one.

Focusing on twelve large cities in the northeastern quadrant of the country (Boston, New York, Buffalo, Philadelphia, Pittsburgh, Baltimore, Cleveland, Cincinnati, Detroit, Chicago, St. Louis, and Minneapolis), Teaford traces the struggle for revitalization from the recognition of the problem of urban decline (resulting from the movement of population, retail trade, and manufacturing jobs to the suburbs and physical obsolescence) in the 1930s through the various strategies employed over the next four decades to reverse this trend. He stops in the mid-1980s when it remained depressingly evident that the mayors and their allies were failing in this task. The story is told almost entirely from the perspective of the individual cities; actions taken in Washington and the state capitals are assumed as "givens," which localities must either work with or around. Teaford's research is stunning: in addition to consulting the relevant secondary literature, he has checked the newspapers and official reports in each of these cities and has looked at obscure professional and trade periodicals. His endnotes are a gold mine of sources for anyone

interested in the political and economic affairs of these dozen municipalities.

Yet this study is as unsatisfying as all of the hype—which Teaford faithfully and skeptically recounts—about the cities' comeback. Part of the problem is that, for most readers of newspapers and weeklies, much of what Teaford has to report is neither fresh nor surprising. The names are familiar (for example, David Lawrence, John Lindsay, Kevin White, Coleman Young), and the issues are usually handled in journalistic fashion. Instead of asking probing questions about the social dimensions of the urban dilemma, Teaford, to a surprising degree, has adopted the agenda of those interested only in physical redevelopment. Teaford tries to distance himself from those political scientists who have tagged urban renewal as little more than a conspiracy among municipal politicians and powerful business interests to rebuild the downtown core for their own narrow purposes, but his attempts to show how neighborhood groups succeeded in making themselves heard are negated by his need to return time and again to proposals for shopping malls and convention centers.

The basic difficulty with the book is that it reflects, rather than rises above, modern American society's inability to decide what role we want our cities to play and how we are to evaluate their performance. Teaford has given us a story—a well-told one, to be sure—but now we need a framework by which we can judge what it all means.

MARK I. GELFAND
Boston College

FRANK FREIDEL. *Franklin D. Roosevelt: A Rendezvous with Destiny*. Boston: Little, Brown. 1990. Pp. viii, 710. \$15.95.

Frank Freidel's biography of Franklin D. Roosevelt is an excellent summary of the life and political career of the most important president of the twentieth century. Drawing on the insights he has gained as the preeminent student of Roosevelt's impact on American history, Freidel offers a graceful narrative that describes the president's private and public actions in a persuasive way. The treatment of World War II, an area that Freidel had not discussed in his previous books on Roosevelt, is fresh and enlightening.

Revelations about Roosevelt's personal life and character have taken some of the glow off his historical reputation. Freidel examines the problems with the Roosevelt marriage with sensitivity, and he gently suggests that Eleanor Roosevelt may have been a burden to the president toward the end of his life. In political terms, Freidel has some interesting things to say about Roosevelt's troubled relationship with the Democratic Party. The president's efforts to move the party toward a more liberal stance serve as a useful reminder that American politics has had a conservative orientation throughout most of this century. The

Democrats in Roosevelt's day still retained an interest in political victory rather than ideological purity that overcame their internal doubts about what Roosevelt asked them to do.

Some of the darker aspects of Roosevelt's record could have had more emphasis. The relocation of Japanese Americans during World War II deserves more than a two-paragraph summary that does not explore adequately Roosevelt's responsibility for the policy. Roosevelt's relationship with J. Edgar Hoover and the Federal Bureau of Investigation is not examined, and the president's record on wartime civil liberties does not receive much scrutiny. Freidel addresses only in passing the extent to which Roosevelt's methods, including recording conversations, wiretapping, and some political dirty tricks, anticipated the imperial presidency of the postwar years.

On several disputed issues Freidel might have elaborated on Roosevelt's role. The treatment of the president's performance regarding Pearl Harbor could have been more assertive in dismissing the charges that he knew about the Japanese attack in advance. Freidel also is rather gentle toward Roosevelt's record on the issue of Jewish refugees and the Holocaust. The sources that he quotes leave the impression that the president shared at least some of the anti-Semitism that pervaded the State Department, Congress, and American society at the time.

The book ends with an evocative treatment of Roosevelt's funeral in 1945. Given the time and thought that Freidel has devoted to the president's life, it would have been useful to have had a concluding essay that assessed Roosevelt's place in American history. Younger scholars, not familiar with Roosevelt as a personality and political leader, may wonder why so many Americans of that generation followed his leadership with such devotion. Conservative criticism of the New Deal and its policies has called into question some of the verities about Roosevelt's record. At a time when biographers of Ronald Reagan compare his political skills and governing style with those of Roosevelt, it would be rewarding to have Freidel's mature wisdom on Roosevelt as a historical figure. An introduction that reviewed the development of Freidel's attitude toward Roosevelt as a biographical subject would also have been enlightening.

This book becomes the best single biography of Franklin Roosevelt and will serve as the most appropriate introduction to the ever-growing literature on his life and times.

LEWIS L. GOULD
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Austin*

BETTY HOUGHIN WINFIELD. *FDR and the News Media*. Urbana: University of Illinois Press. 1990. Pp. xiv, 276. \$34.95.

The presidency of Franklin D. Roosevelt represents an important way station on the road to a managed media in the United States. The issues involved in exerting control over a free press, and the Roosevelt administration's responses to them, were not new in 1933; they had already been raised and the foundations for addressing them laid a generation earlier during the presidencies of William McKinley, Theodore Roosevelt, and Woodrow Wilson. The executive's public relations bureaucracy, on which day-to-day news management depends, was created long before FDR became president, as were such staples of presidential leadership as White House press releases and press conferences. Roosevelt's contributions to the process were a refinement of technique, particularly visible in his hundreds of press conferences, and his mastery of the still-new political medium of radio, with its advantage of providing direct access to the public.

As Betty Houchin Winfield makes clear, the relationship of the modern presidency to the press raises difficult issues concerning the control and dissemination of information in a democracy. Roosevelt, of course, did not worry much about these issues; his interest was in bending media coverage and interpretation to his own purposes, and he blithely assumed that all would work out for the best if he succeeded. The danger to democracy, as he saw it, lay in the potential failure of the national will to conform to his leadership, not in the short circuiting of the political process through his withholding and manipulating information. The press was more ambivalent, torn between the reporters' desire for the whole story and the loyal citizens' willingness to be coopted in the name of the greater national good. Thus, although never quite succumbing to administration press agent manipulation, the media maintained a relationship with the chief executive that was more congenial than adversarial. Winfield points out that the reporters' resistance to the president's management of information weakened as the focus of national attention shifted from domestic issues to foreign concerns. As the New Deal gave way to World War II, the press struggled to find some answer to the ambiguous question of what constituted a legitimate secret and what should be reported in the news. Its answer, more often than not, reflected its willingness to place the requirements of victory, including the maintenance of good public morale, above the needs of a free press and an informed public.

Winfield's own views reflect a similar ambivalence. She frequently defends the idea of an unfettered and curious press as essential to a well-informed public, particularly on domestic issues. But she accepts the principle of censorship during the war and writes that "to tell the truth in a democracy during an all-out war would undercut the official wartime policy and possibly jeopardize the war effort" (p. 166). A press that wholeheartedly accepts such a position on telling the truth has lost more than just its independence; it has

also surrendered its function as the guardian of free opinion in a democracy. It seems astonishing that Winfield, a professor of journalism history, should prove willing to give this function away, even fifty years after the events in question.

ROBERT HILDERBRAND
University of South Dakota

WALTER L. CREESE. *TVA's Public Planning: The Vision, the Reality*. Knoxville: University of Tennessee Press. 1990. Pp. xxi, 388. \$38.95.

In the opinion of many historians, the Great Depression of the 1930s gave the U.S. government the opportunity to reset the nation's economic and social agenda, to experiment with untried theories, and to put idealistic notions into practice. That the New Deal did not fully realize these aspirations, therefore, has been the source of much disappointment to these historians. Walter L. Creese's well-written and comprehensive book on the architecture and public planning of the Tennessee Valley Authority (TVA) is very much in this vein. An excellent chapter on the ideas and images behind TVA is followed by the author's assessment of how the agency's alternative vision of what American life might be like gradually was chipped away until the ultimate reality was considerably different from the dream.

In terms of both architecture and planning, Creese believes that the juxtaposition of technology and nature in the town and dam site of Norris, Tennessee, was never equaled by TVA. The perfect coordination of architecture, engineering, and landscape architecture, Norris Dam was described by economist Stuart Chase as a structure of "clean, strong walls between the hills of pine." Yet even here, as Creese shows, the complete dream was never realized. Job training for the area's people and an economy based on small industry, handicrafts, and part-time farming never really got off the ground, killed by TVA director David Lilienthal. Once co-director Arthur Morgan was removed by President Franklin Roosevelt in 1938, Lilienthal became the dominant figure in TVA and the attempt to build a utopian community next to a dam site was never repeated. The town of Norris was sold to private developers in 1948.

TVA's architectural designs for its dams and powerhouses have won prizes from architects, but Creese takes a somewhat different view. Although agency architects (bolstered by the hiring of Roland Wank) spoke of a "bright new order" of architecture that would be clean and powerful, in the end too much was borrowed from the Art Deco or Moderne architectural impulses, both of which offered dramatic contrasts to the environment rather than trying to blend into it.

Finally, Creese maintains that the pragmatic considerations of producing cheap electricity simply overwhelmed the visionaries, thus dooming TVA to a

mission that was too narrow. To be sure, this is a story that has been told before, but here it is told in a fresh way, from the perspective of an architectural historian.

As to the "host region" itself, Creese seems to be either unfamiliar with or unconcerned about the historiographical battle under way over the nature of the Appalachian population at the time TVA entered (or intruded into, some would say) the region. The author seems content with TVA's own characterization of these people as a premodern, dysfunctional people, a characterization that would allow the agency to claim that it "saved" the Tennessee Valley. Readers who object to that characterization should be forewarned, although it does not detract from the major argument of the book.

In sum, Creese has given us a well-thought-out, artfully written, and intelligently illustrated assessment of TVA and the gap between the agency's vision and the reality.

WILLIAM BRUCE WHEELER
University of Tennessee,
Knoxville

ALICE M. HOFFMAN and HOWARD S. HOFFMAN. *Archives of Memory: A Soldier Recalls World War II*. Lexington: University Press of Kentucky. 1990. Pp. xv, 199. \$25.00.

Alice M. Hoffman, an oral historian, and her husband Howard S. Hoffman, a psychologist and World War II veteran, have produced a significant work in oral history. A case study of memory, with references to appropriate psychologists' studies, this book will help those who collect and use oral histories understand how and what people remember.

Howard Hoffman was one of numerous young men who fought in Italy and France and was among the troops that occupied Germany after the war. The book, however, is only tangentially about World War II. It provides a convenient set of memories to explore because there is a defined period of time, from Howard's enlistment in 1943 to his homecoming in 1945.

The Hoffmans explore these memories systematically. Although he had recounted some stories often, Howard had never previously recorded his military career. In 1978, Alice conducted her first interview, leading him chronologically through his memories. Howard then tried not to think about the war or rehearse his memories in any way. Alice, meanwhile, found a log from Howard's company in Europe that allowed her to compare the official contemporary record with his account. In 1982, she conducted a second interview on the same topics but sometimes broke the chronological progression of questions to see if the stories recorded would differ. To check Howard's memories the Hoffmans used traditional

historical sources, written and visual; took a trip to Edgewood Arsenal, where Howard was stationed; conducted interviews with other members of his company; and attended a battalion reunion.

The book relates the initial interview of 1978 and analyzes how traditional sources supported or refuted his memories. Appendices include part of the interview from 1982 and part of the company's daily log. The Hoffmans initially did not realize how much written and visual material would be available, but Howard could not recall some events, even when shown his photograph. Still, this study was certainly enhanced by concentrating on events that could hardly be totally erased from memory.

The Hoffmans conclude that there is a category of memory that they define as archival, "a separate class because it has its own set of principles or laws . . . [I]t is unaffected by disuse, is susceptible to little or no decay over time" (p. 150). Archival memory can be readily recalled because it is rehearsed, but it is difficult to retrieve lost events with prompts such as photographs, perhaps because these events were overshadowed at the time by something more memorable.

The study of memory is not new to historians or psychologists. Barbara Allen and Lynwood Montell raised some of the same issues in *From Memory to History: Using Oral Sources in Local Historical Research* (1981). Historians such as David Thelen and Michael Frisch are also interested in how we remember the past. This book is an important addition to that literature because it records a systematic experiment. Not surprisingly, we find that, while some events may be forgotten, oral history still provides an excellent way to capture emotions and personal insights.

BARBARA J. HOWE
West Virginia University

HOLLY COWAN SHULMAN. *The Voice of America: Propaganda and Democracy, 1941-1945*. (History of American Thought and Culture.) Madison: University of Wisconsin Press. 1990. Pp. ix, 282. Cloth \$37.50, paper \$12.95.

People interested in America's propaganda efforts during World War II now can consult two good books on the subject: Allan M. Winkler's *The Politics of Propaganda* (1978) and this work by Holly Cowan Shulman. Whereas Winkler's study covers the full range of domestic and foreign activities of the Office of War Information (OWI), Shulman focuses on the Overseas Branch, and especially on the French-language radio broadcasts of the Voice of America (VOA). The books are complementary in another sense: Winkler's excels in showing where OWI fit into the larger context of wartime American culture and politics, while Shulman's offers better detail on the personalities involved and the types of programs broadcast. A real strength of Shulman's study is the

insight gained from interviews with approximately twenty-five former OWI staffers, many of whom had worked in the Overseas Branch with her father, Louis G. Cowan.

The basic argument of the book is straightforward. The early leaders of the Overseas Branch were fervent New Deal liberals who saw the struggle in Europe as an effort to extirpate fascism in all its forms. Much of the programming in 1942 and early 1943 reflected this crusading zeal: "The Voice of America was agitprop on the air, provoking the masses to resist" (p. 59). In late 1942 and 1943, however, the U.S. policy of working with Nazi collaborators in French North Africa and fascists in Italy undercut the propagandists' vision and forced them to modify their message. Well before three high-ranking liberals angry about the direction of U.S. foreign policy were fired in early 1944, VOA programming largely had shifted from antifascist agitprop to factual news stories designed to encourage victory, not political transformation. For the remainder of the war, a chastened VOA reflected the increasingly conservative mainstream views of official Washington.

Without intending to do so, Shulman raises serious questions about whether the tens of millions of dollars devoted to the Overseas Branch were well spent. By spring 1944, it was clear that most Europeans who had been tuning in foreign broadcasts had been listening to the British Broadcasting Corporation (BBC) rather than to VOA; one OWI study found that at least nine French listeners relied on BBC to every one who tuned in VOA (p. 176). Unlike the BBC broadcasts from London, which came in loud and clear, the short-wave VOA broadcasts from New York were hard to pick up. According to refugees from the Continent, the content of BBC broadcasts tended to be superior as well.

As Shulman demonstrates in an epilogue, since 1947 substantial expenditures have continued to be made on the VOA and on other information programs abroad. Nowhere in her book does the author provide evidence that would serve to justify such outlays.

RALPH B. LEVERING
Davidson College

JOEL H. ROSENTHAL. *Righteous Realists: Political Realism, Responsible Power, and American Culture in the Nuclear Age*. (Political Tradition in Foreign Policy Series.) Baton Rouge: Louisiana State University Press. 1991. Pp. xxii, 191. \$24.95.

The prophets and policy makers who were the famous "realists" of U.S. foreign policy in the 1940s and 1950s continue to fascinate many of us. Intellectual historian Joel H. Rosenthal recounts the story of five diverse and well-chosen participants: Dean Acheson, George Kennan, Walter Lippmann, Hans Mor-

genthau, and Reinhold Niebuhr. He approaches them in a spirit that seeks to be faithful to the perspective and contribution of each of these distinct men, while undertaking at the same time to discuss "realist" thought as a whole. The great strength of Rosenthal's book is that he presents "realism" and these realists in the most straightforward manner possible. He provides us with a nicely focused and concise book that knows where it is going and goes there. There are but few frills and scant ideological special pleading in a realm where there has been no shortage. The book fully succeeds in articulating the views of its five subjects and the history of realism in U.S. foreign policy. It constitutes an excellent, reliable, and welcome guidebook to "realism" in American intellectual history.

Rosenthal, of course, has his interpretations, even as he desires to limit his presence in the text and instead accent that of his subject and his subjects. For example, he insists that Morgenthau, Kennan (who appears to engage Rosenthal the most and receives the largest attention), and the others changed their outlooks little in their careers, certainly not in opposing the Vietnam War. This is a claim they would welcome, but it is clearly controversial. He also argues that the "realists" very much affirmed the ethical realm, despite charges to the contrary by numerous critics. They recognized morality as a factor in international relations, but more to Rosenthal's point, they did so through their own values and their allegiance to the United States and the goal of "responsible use of power." Of course, it is one thing to say that "realists" took morality into account as one element in international relations or that they had their own moral perspective; as Rosenthal himself recognizes, it is another to say that morality was genuinely significant for the conduct of foreign policy or that they respected other-regarding moralists in international affairs.

Reading this book takes one back to other times and eras, and Rosenthal might have set his thinkers more broadly in the American political tradition and the 1940s and 1950s in particular. Reading Rosenthal also suggests that possibilities are opening up today for reflective reexamination of foreign policies of twenty-five years and more ago, guided by something other than a total absorption with Vietnam. Rosenthal's study of the "realists" helped me to do so. I recommend this work to anyone who wants to explore the thought and thinking of "realism" in its heyday in American intellectual life.

ROBERT BOOTH FOWLER
*University of Wisconsin,
Madison*

RANDALL B. WOODS and HOWARD JONES. *Dawning of the Cold War: The United States' Quest for Order*. Athens: University of Georgia Press. 1991. Pp. xiii, 335. Cloth \$35.00, paper \$14.95.

It comes as something of a surprise to realize that there has been no comprehensive American history of how the Cold War began since Thomas G. Paterson wrote *On Every Front* in 1979. Although the intervening years have seen a remarkable amount of monographic research on various aspects of that once-controversial topic, no new interpretive synthesis has as yet appeared. The end of the Cold War makes the need for such a synthesis all the more urgent, for knowledge of how the struggle finally came out is bound to reshape our understanding of what started it, and why it took the form that it did.

Randall B. Woods and Howard Jones have set out to fill this gap by writing the first post-Cold War history of Cold War origins. They have succeeded only up to a point. Their book is indeed a comprehensive and balanced synthesis of recent scholarship on American and British policy with respect to Europe from 1945 through 1949. Other Western participants in the post-World War II anti-Soviet coalition get little attention, however, and developing Cold War tensions elsewhere—notably in East Asia—are ignored altogether. The authors do attempt to incorporate Soviet perspectives into their account, but they have done so almost entirely on the basis of Western sources. Their conclusions therefore are likely to require modification when (and if) Soviet documentation becomes available.

Woods and Jones claim to have arrived at an interpretation that is “clearly orthodox, or, to use another term, realist” (p. xii). Yet for the most part they follow familiar lines of postrevisionist scholarship by viewing the defensively motivated but still threatening expansionism of the Soviet Union under Joseph Stalin as the principal cause of the Cold War; the emphasis on the ideological roots of confrontation that characterized most orthodox accounts is missing from this one. The Soviet challenge, they insist, was “real, intense, and immensely dangerous,” but despite occasional excesses, Western reactions to it in the form of the Truman Doctrine, the Marshall Plan, the Berlin airlift, and the North Atlantic Treaty Organization “were measured responses designed to contain Communist expansion without provoking full-scale military conflict” (p. vii, xii).

This argument makes sense as far as it goes, but in the light of recent “corporatist” and “hegemonic” interpretations of American foreign policy during the early Cold War many readers will find its geopolitical and Eurocentric focus too narrow. As the book’s own subtitle implies, the United States also had a vision of a postwar world order; that vision reflected deeply rooted American interests and was not simply a response to Soviet behavior. Corporatist historians have raised important questions about the extent to which this order served the interests of nations and peoples elsewhere in the world; the collapse of communism has raised the same question from an alternative perspective that may well yield different answers. Post-Cold War historians of Cold War origins

are going to have to wrestle with this large and complicated issue. Unfortunately—and despite their subtitle—Woods and Jones do not.

Theirs is, however, the most satisfactory narrative history we now have of how the Cold War came about in Europe. It gives full attention to the strains that existed in Anglo-American relations during the late 1940s, and especially to President Harry S. Truman’s curiously persistent aspiration to serve as a mediator between London and Moscow. It is quite good on the interaction of domestic and foreign policy in both the United States and Great Britain. It provides a first-rate account of the German question, the Berlin blockade, and its aftermath. The book also makes the useful point that, considering how the Cold War ended in Central and Eastern Europe, it is going to be more difficult than ever to sustain the old revisionist argument that U.S. policies in that part of the world began it.

But because of its failure to look at the Cold War in a global context, and because it has bypassed some of the most interesting questions raised by Cold War scholarship in recent years, this book does not quite live up to the ambitious claims its authors make for it.

JOHN LEWIS GADDIS
Ohio University

RICHARD W. HULL. *American Enterprise in South Africa: Historical Dimensions of Engagement and Disengagement*. New York: New York University Press. 1990. Pp. xviii, 419. \$20.00.

This narrative survey by Richard W. Hull of the economic relationships between the United States and South Africa is well written and well informed, and it is unique in focusing on economic ties over the entire period from 1680 through 1987. As such, it brings together a wealth of material otherwise unavailable in an organized form. The work belongs to the genre of business history and each chapter weaves together macroeconomic trends with accounts of the roles of specific individuals and companies. Roughly half of the text deals with the period through 1929, and the remainder brings the story up through the immediate reaction to the Comprehensive Anti-Apartheid Act of 1986.

In the first period, Hull’s major focus is on the rise and fall of trade: of U.S. raw materials and later manufactures to South Africa, of South African agricultural products, and then above all of minerals, to the United States. A subtheme is the dual role of British business interests as both intermediary between the United States and South Africa and as competitors to the United States. In the second period, the themes include the rise of U. S. direct investment in South African mining and manufacturing, the increasing importance of loans in the 1970s and thereafter, and the response in the post-Soweto

period in particular to demands for sanctions and disinvestment.

Although Hull deliberately stays away from systematic investigation of political or diplomatic issues, his definition of enterprise is expansive. He fits cultural, missionary, and foundation involvement into the narrative, as well as the impact of government policies in both countries (U.S. attitudes during the Anglo-Boer War, for example, and the effect of post-1948 apartheid education policies in reducing the U.S. missionary presence). Whatever the area of one's concern in studying U.S.-South African relations, Hull's book is a good place to look for documented references to aspects one may have missed. One will not find, however, much attention paid to more theoretical discussions on the South African political economy or the relationship of U.S. foreign economic relations with foreign policy more generally.

The primary focus on economic engagement serves Hull best in the early period, when South African racial policies were a nonissue for the primary economic actors on both sides, and other political issues (the British-Boer dispute) were of minor importance. His treatment of the impact of the Cold War and the anti-apartheid movement on U.S.-South African relations is sketchy, revealing primarily the paucity of scholarly research on the topic, but there is more material available than shows up in Hull's bibliography (for example, Janice Love, *The U.S. Anti-Apartheid Movement* [1985] and David Hauck et al., *Two Decades of Debate: The Controversy over U.S. Companies in South Africa* [1983]). Among new studies appearing are Thomas Borstellmann's "Apartheid, Colonialism and the Cold War" (PhD diss., 1990), which shows that the strategic importance of South African uranium was probably more important than the short account given by Hull.

The dispersed character of the anti-apartheid movement and the divestment campaigns, with their multiplicity of short-lived local organizations, probably means that much new research is needed before any good historical overview can be written. But Hull's account is misleadingly brief on the role of black activism in the 1970s and in general probably overemphasizes the role of national organizations. He does not mention the role of the African Liberation Support Committees in the early 1970s. A minor error is that Hull credits the American Committee on Africa with forming the Southern Africa Committee and its magazine *Southern Africa* in 1972, whereas that committee and its magazine in fact emerged from the National Student Christian Federation in 1964-65.

WILLIAM MINTER
American University

IRVING BERNSTEIN. *Promises Kept: John F. Kennedy's New Frontier*. New York: Oxford University Press. 1991. Pp. viii, 342. \$24.95.

In this lively account of President John F. Kennedy's domestic policy achievements, Irving Bernstein aims to settle a historiographical score. Bernstein minces no words: "The naked conclusion is that he was a very successful President, that the revisionists were dead wrong" (p. 7). Although I do not think Bernstein proves his argument as well as he sets it out, he has written a fascinating synthesis of domestic policy making in the Kennedy White House.

Bernstein presents a traditional and narrow political history. This is no story of increasing state capacity, policy feedbacks, prefigurative politics, street heat, changing constructs of citizenship, or any other expanded frame for explaining the politics of the 1960s in historical terms. Bernstein leaves out the more unpleasant side of the Kennedy domestic policy, such as the approval by the Kennedy brothers of J. Edgar Hoover's campaign against Martin Luther King, Jr. Bernstein focuses on the president, his staff, and Congress and the public policies that they struggled over. In particular, Bernstein looks at civil rights, tax policy, the minimum wage, pay equity for women, labor and unemployment issues, federal aid for education, Medicare, and the creation of the Peace Corps.

Kennedy, Bernstein writes, learned on the job about domestic issues and policy making, especially economics. He argues that by 1963 Kennedy knew what needed to be done and how to do it. Bernstein concludes that "when [Kennedy] predicted on November 14, 1963, that his entire program would be enacted within eighteen months, he was right" (p. 298).

To a large degree, Bernstein tells a familiar story but from the perspective of the post-Reagan rather than the post-Johnson years. For example, he does not contend that Kennedy was any more forward thinking or enthusiastic about civil rights issues than the revisionists argue. But he emphasizes that when push came to shove, Kennedy moved a comprehensive civil rights bill forward—the glass is not half-empty but half-full.

In addition, Bernstein wants contemporary readers to understand just how difficult it was to move liberal legislation forward in the early 1960s. Bernstein is at his best here, giving an evocative picture of congressional hardball, administration ball carriers, and the fast play of interests. Again, rather than focusing on Kennedy's early uncertainties about the legislative process, he emphasizes the breakthroughs such as Kennedy's fight to expand membership on the House Rules Committee. As Kennedy told his congressional liaison Larry O'Brien, "We can't lose this one . . . the ball game is over if we do" (p. 283). Kennedy won, and his quiet victory on the rules committee paved the way for the legislative breakthroughs of the middle 1960s.

Despite Bernstein's spirited defense, the Kennedy he limns strikes me as a reluctant domestic policy warrior with a mixed record. Many of Kennedy's

successes were long-term Democratic party goals that were pushed forward by congressional leadership or savvy administration figures (such as Esther Peterson) operating with limited presidential support. Still, caveats aside, Bernstein gives us a rich picture of the Kennedy domestic record.

DAVID FARBER
Barnard College

PAUL R. HENGGELER. *In His Steps: Lyndon Johnson and the Kennedy Mystique*. Chicago: Ivan R. Dee. 1991. Pp. vii, 325. \$27.50.

Paul R. Henggeler's psychologically oriented examination of Lyndon B. Johnson's reactions to the personae and legacies of John F. Kennedy and Robert F. Kennedy is a well-researched but not especially provocative book. Anyone familiar with the existing literature on the Kennedy presidency and on Johnson will find few new interpretations here, but this work is a credible scholarly survey of territory that already has been thoroughly examined.

Henggeler offers appropriate and measured criticisms of earlier interpretations of Johnson put forward by Doris Kearns and by Robert Caro, but in the end he plows very little new ground of his own. Demonstrations of Johnson's "erratic mood swings" (p. 57) and "chronic insecurities" (p. 85) are already familiar, as is Henggeler's focus on how President Johnson made good use of John Kennedy's aura while also feeling overshadowed by the legacy he partially inherited. Johnson's highly ambivalent feelings about the Kennedys predated the controversy at the 1960 Democratic National Convention over Johnson's selection as the vice presidential nominee, but Johnson's understandable resentment over Robert Kennedy's opposition to his nomination intensified rather than weakened over time. From 1960 onward—both before and after November 22, 1963—Johnson "divided 'Kennedy' into two distinct entities—the 'good' John and the 'bad' Bobby" (p. 61), denying any differences with the first and perceiving only intense animus from the second. It was a deleterious mind-set, one that Henggeler believes played a major role in Johnson's "self-destruction" (pp. 22, 244).

One expects Henggeler—especially in light of the book's straightforwardly psychological opening chapter—to conclude his study with a clear and explicit discussion of how Johnson's attitudes toward the Kennedy brothers either reflected and/or magnified Johnson's undeniably large and self-damaging insecurities, but no such analysis appears. Henggeler traces the political decline of the Johnson presidency between the summer of 1965 and the spring of 1967 (when Robert Kennedy, as junior Senator from New York, publicly broke with Johnson over the Vietnam War), but his productive mining of documents from both the Kennedy and Johnson presidential libraries

fails to save him from a number of distracting errors. Two of the most striking are "the Office of Employment Opportunity Act" (pp. 117–118) and "the Equal Economic Opportunities Administration" (p. 168).

Henggeler is unabashedly sympathetic to the political predicament in which President Johnson found himself, but his decision not to offer any extended discussion of Johnson's mind-set with regard to the Vietnam conflict leaves a reader wondering whether this book is incomplete rather than necessarily selective in its treatment of Johnson's political decline. Henggeler readily concedes that Johnson, unlike Kennedy, was someone who "inspired little affection when he was alive and little emotion in death" (p. 254), but he regrets that Johnson's "vast achievements were obscured by an unpopular political style" (p. 123) and commiserates at how Johnson "tried to surpass Kennedy in terms of substance when presidential success was becoming, increasingly, a matter determined by image" (p. 122).

Although Henggeler once again never fully or explicitly develops his stance, a reader finishes the book surmising that the author believes Johnson's political failure in the presidency was less the result of disastrous policy choices than of personal circumstances that someone with Johnson's psyche was unable to surmount. "Unlike John of the mythic past and Robert of the mythic future, Johnson was condemned to the present, wedged between two illusions and forced to deal with harsh realities" (p. 250). Had Henggeler developed his interpretations further, his study could have been far more challenging than it is.

DAVID J. GARROW
The Twentieth Century Fund

LAURA KALMAN. *Abe Fortas: A Biography*. New Haven: Yale University Press. 1990. Pp. xiii, 499. \$29.95.

Laura Kalman's work is the first full biography of Abe Fortas, the supreme court justice. Drawing on previously unavailable personal papers, on numerous archives, and on the recollections of his family, friends, and even his detractors, Kalman painstakingly examines the legal and constitutional controversies of the 1950s and 1960s, when many thought American society was coming unhinged. On another level, Kalman exposes the attacks on liberalism in this nation, particularly the liberalism of the Warren Court.

Fortas was born in Memphis and attended Yale Law School, where he and his brilliance blossomed. It was at Yale that Fortas met his teacher, friend, mentor, and later his colleague on the Court, William O. Douglas. Also while at Yale, Fortas encountered the new "legal realism" school, led by Jerome Frank and Douglas, which advocated the use of the sciences and the social sciences to arrive at "good" decisions rather than reliance on abstract legal principles derived from the study of case law only. Thus, at Yale, Fortas was "provided with a jurisprudence to guide

him, and with mentors who would propel him to power" (p. 26). Like Douglas, Fortas treated law as an instrument of social policy. And for Fortas, New Deal liberalism was the political analogue of legal realism. Law was the "great engine of social change" (p. 29).

Fortas worked as a lawyer in several of Franklin Roosevelt's New Deal agencies, but in 1945 he went into private practice, where he remained for the next twenty years. By the 1960s, Fortas was an influential Washington insider and adviser, particularly to Lyndon Johnson, who in 1965 named him to the Supreme Court. The heart of this biography concerns the controversy over his nomination as Chief Justice in June 1968, which began a series of events that led to Fortas's fall. In brilliant style and analysis, Kalman takes us through the daily proceedings of the Senate Judiciary Committee. Opposition to his nomination was swift and brutal. Southern senators opposed him because he was a Jew, a New Deal liberal, and a member of the Warren Court, which had handed down the *Brown v. Board of Education* decision, decisions on freedom of speech that seemingly gave approval to pornography, and favorable decisions respecting the rights of criminals. The Warren Court was, to many, a court of infamy, and Fortas was its beau ideal. Further opposition to the nomination came because of charges of impropriety. While a justice, Fortas had advised President Johnson on the war in Vietnam, civil disobedience, and other clearly political matters. Fortas had, his opponents argued, violated the principle of separation of powers. Senators Strom Thurmond, Sam Ervin, and John L. McClellan painted Fortas as a crony who could hardly be expected to be objective as Chief Justice. Kalman uses the personal papers of Fortas, the Judiciary Committee's proceedings, the *Congressional Record*, and articles from the leading newspapers and magazines in her analysis of this episode. Fortas was portrayed as an immoral, arrogant, self-seeking opportunist and lost the nomination. His troubles, however, were not over. Evidence surfaced that Fortas had received money and a lifetime contract from the Wolfson Foundation. In the end, he not only did not become the Chief Justice but he also was forced to resign from the Court and lost his position at the law firm.

Kalman suggests that there was more to his demise than just these improprieties. What brought Fortas down was what he was and what he stood for. He was a Jew, a liberal, a friend and adviser to an unpopular president escalating an unpopular war, and a liberal member of what was perceived by many to be an activist Court. Fortas was, in a word, the personification of all that was wrong in America during the 1960s. By destroying Fortas, conservatives hoped to strike a blow at liberalism in America. One wishes Kalman had given more attention to this larger reason for Fortas's defeat.

Kalman is a clinical and dispassionate researcher of Fortas's life. His sympathies and beliefs were funda-

mentally steeped in liberalism—civil rights, welfare, and criminal rights—yet his private behavior was nothing less than despicable. He seemed to have betrayed the same liberal agenda that he espoused, and possibly the American people. He probably was a crook and, as Kalman points out, ironically was guilty of behavior that he himself would not have condoned in others. Kalman describes Fortas's many complexities but she does not reveal an adequate explanation of his darker side. After conceding that he perhaps deserved an attack from conservatives, Kalman leaves the reader wondering why Fortas allowed himself to be put in situations that left him so vulnerable. Was he naive? He had been in circles that should have precluded such naivete, and Kalman might have pursued this theme more thoroughly. What was his moral definition? As an advocate for "the least among us" and unpopular causes, Fortas was obviously a fighter, a man of great courage and, at times, considerable prudence. He seemed deficient, however, in common sense. Why? With his political savvy, Fortas should have known that the power and prestige of the Supreme Court could not have suffered his improprieties, or even the hint of them. This story is the odyssey of a man and a movement. To see all of his ambitions and achievements reduced to scandal was a tragedy, almost too rich for the historian.

Kalman's account is unbiased and balanced. Her use of materials from friends and family might cause scholars to question the objectivity of this approach. But she received and used those materials carefully. Her biography is must reading for students of both judicial review and the turbulent 1960s. Moreover, her study gives a view from the top of the problems and frustrations of the Johnson administration. Finally, Kalman's biography is timely. The Court and the era of Fortas certainly marked the beginning of the downward spiral of liberalism in America.

There are a few structural weaknesses that might be mentioned. A bibliography would have better served her readers; wading through hundreds of endnotes is disconcerting. Kalman's prologue would seem to suggest an epilogue, but there is none. And historians may find her use on several occasions of confidential sources problematic.

OLIVE A. TAYLOR
Howard University

JOSEPH N. TATAREWICZ. *Space Technology and Planetary Astronomy*. (Science, Technology, and Society.) Bloomington: Indiana University Press. 1990. Pp. xvi, 190. \$29.95.

This is an excellent record of the beginnings of the NASA planetary astronomy program in the years 1958–70. Joseph N. Tatarewicz opens with a good sketch of early ground-based planetary research at Lowell Observatory, but this section is flawed by his acceptance of the myth that professional astronomers

elsewhere in America virtually ignored the astrophysical study of the planets. Important papers and books by such pillars of the astronomical establishment as W. W. Campbell, Walter S. Adams, Henry Norris Russell, William H. Wright, and Seth B. Nicholson demonstrate the incorrectness of this view, although undoubtedly more work should have been done. The main problem is that before World War II new or improved instruments were only very rarely put into use. Astronomers did tend to turn them on the planets, but once a good high-dispersion spectrum, bolometer measurement, or series of infrared photographs had been obtained of each bright planet at a favorable opposition or quadrature there was little more to do, in comparison with the universe of discoveries waiting to be made in the stars and nebulae.

The opening of the space age after Sputnik changed all that. Politicians and generals were eager to pour money into satellites and space vehicles designed to travel to the moon, then on to the planets. Hard data were needed, particularly on planetary atmospheres, to design the vehicles and plan the flights. Brilliant research scientists, including astronomers, geophysicists, physicists, and chemists, eager to build new instruments, make new measurements, and gain new knowledge pushed forward, impatient for the resources to carry out their dreams. A layer of scientific bureaucrats, not so brilliant, soon arose between the researchers and the politicians, trying to interpret each group to the other. The scientists wanted long-term commitments to a growing program of fundamental research; the government officials wanted immediate results and were driven by political and economic pressures into sudden shifts of priorities and increases and decreases of funding. In the end America's present strong planetary science establishment emerged, based on government-financed laboratories closely associated with universities, but it was hard going all the way.

Tatarewicz tells this story well. He is at his best in his descriptions of the scientific problems and the human interactions involved. His study is based on thorough research in NASA, Jet Propulsion Laboratory, and other government archives, and on numerous interviews and transcripts. The book would have been improved by including more information on the educational backgrounds and research accomplishments of the scientists and NASA administrators who played key roles. Astronomers who lived through the period knew most of these details, but the reader will not, and as a result may not understand the interrelationships.

The book concludes with a series of graphs and tables and discussion of them "from several theoretical perspectives of the social studies of science and technology" (pp. 117-42). The numbers are interesting, but the theories seem forced and un insightful to the reader familiar with astronomy. Overall, this book

adds greatly to our knowledge of planetary astronomy in the post-Sputnik era.

DONALD E. OSTERBROCK
Lick Observatory
University of California,
Santa Cruz

WALTER G. VINCENTI. *What Engineers Know and How They Know It: Analytical Studies from Aeronautical History*. (Johns Hopkins Studies in the History of Technology.) Baltimore: Johns Hopkins University Press. 1990. Pp. viii, 326. \$45.00.

Walter G. Vincenti is using history to help himself understand his own career. For eighteen years he was an aeronautical engineer at the Ames Aeronautical Laboratory, now NASA's Ames Research Center. Thereafter he was professor of aeronautical engineering at nearby Stanford University, his alma mater. An early and continuing member of Stanford's innovative Program in Values, Technology, and Society, Vincenti came into contact with scholars from other disciplines who were, like him, interested in understanding more fully the nature of science and technology and their place in society. He has studied the history of technology with Rupert Hall at London University, and he has collaborated with economic historian Nathan Rosenberg on a very successful study of the Britannia Bridge (1978).

Since then, Vincenti has been working on the case studies that make up the bulk of this book. All five have been published previously, four of them in *Technology and Culture*. They were originally conceived and executed to stand alone, but as the work proceeded Vincenti began to develop a greater understanding of and appreciation for the epistemology of engineering knowledge. All his case studies bear on this issue. His understanding of it proved different from anything he found in the existing literature, and his thought finally coalesced in a model that makes clear for him what he has been doing and thinking all these years. Hence, the present volume.

The focus of this study is on engineering design, especially late-twentieth-century aeronautical engineering design. Vincenti ignores production and operation, the other branches of modern engineering, although he believes that his conclusions are applicable to those activities as well. He also ignores engineering and technology before the twentieth century, and for the most part technologies outside aeronautics, although he suggests that his model may well be universal enough to encompass all that.

After a brief introduction on engineering as knowledge, he presents his five case studies. He examines the Davis Wing, the development of flying-quality specifications, control-volume analysis, early propeller tests, and flush riveting. Each of these topics serves as a vehicle for introducing some aspect of design engineering. The chapter on propeller tests, for

example, demonstrates how two engineers used parameter variation both to gain fundamental knowledge about the nature of airfoils and also to develop a family of propellers from which aircraft designers could choose. Parameter variation is a technique in engineering research with wide application outside the narrow context in which it is examined here.

All the case studies are technical and dense, but they are logically argued and clearly presented, accessible to the lay reader who is willing to invest the time and energy to stay with them. They are followed by a categorization that plots six kinds of design knowledge against seven engineering activities from which new knowledge arises. The resulting matrix then becomes the basis of a "variation-selection model for the growth of engineering knowledge," which Vincenti has derived from the work of psychologist Donald Campbell (p. 241).

This is a book that could have been written only by an engineer. With it, Vincenti places himself in the distinguished company of Eugene Ferguson, Samuel Florman, Henry Petroski, and other engineers who have turned their hands and their insights to the humanities and social sciences. Like them he seems to be driven primarily by the quest to understand his chosen profession. In the process he opens that profession up to lay readers in ways that only initiates can.

ALEX ROLAND
Duke University

CANADA

ANGUS McLAREN. *Our Own Master Race: Eugenics in Canada, 1885–1945*. (Canadian Social History Series.) Toronto: McClelland and Stewart. 1990. Pp. 228. \$15.95.

Since the publication of Mark Haller's *Eugenics: Hereditarian Attitudes in American Thought* (1963), professional historians have published a remarkable number of excellent works on the history of eugenics and scientific racism in the English-speaking world. Recent notable works in the literature include Daniel Kevles's *In the Name of Eugenics: Genetics and the Uses of Human Heredity* (1985), Raymond Fancher's *The Intelligence Men: Makers of the IQ Controversy* (1985), and Barry Mehler's study of the eugenics network in the United States (Ph.D. diss., Washington University, St. Louis, 1987). Although a relatively small number of individuals were enlisted in the eugenics movement, they included influentials in the rise of such learned professions as medicine and social work and key figures in the creation of the academic research establishment in the United States. Although eugenicists failed to gain firm political support for their programs of social control, they did shape the discussion of policy issues ranging from the sources of social dependency to the restriction of mass immigration to the sterilization of persons deemed mentally defi-

cient. Their concerns and vocabulary persist in contemporary debates over the relative importance of heredity and environment in human affairs and in our efforts to explain inequality among ethnic groups.

Given the quality and quantity of studies on the eugenics movement, one might question the need for a study of the movement in Canada. Canadian eugenicists got their intellectual inspiration from England and the United States, but they were responding to local issues as well. Angus McLaren's history of eugenics in Canada demonstrates how a case study of a relatively minor group of social activists can be used both to add important detail to a well-developed historiography and to test issues that are still being contested within the reigning paradigm.

The Canadian example confirms our understanding of eugenics as a progressive reform movement led and supported by academic activists in medicine, public health, social work, and other university-based disciplines, especially biology and psychology. Canadian eugenicists envied the legislative successes of colleagues in the United States who got laws through state legislatures that sanctioned sterilization of "defectives" and who gained the ear of influential congressmen in the fight to restrict immigration. Canadians were less successful on the legislative front, but McLaren argues that they did succeed both in enhancing the power of aspiring professional elites and in popularizing concepts of biological determinism that continue to influence public policy debates. Among the historians of eugenics, McLaren provides one of the clearer demonstrations of the specific influence that the movement exerted, and he proves that influential leaders of the movement persisted in their large claims for hereditarian determinism despite advances in genetics that might have led them to qualify their claims. He also provides vivid portraits of a number of Canadian eugenicists of international stature, including Madge Thurlow Macklin, the founder of medical genetics; Helen MacMurchy, a major figure in the movement to improve maternal and infant health; and William Hutton, the "Canadian father of fluoridation." McLaren's able history reminds us of the many reasons why the last good book on the eugenics movement may never be written.

JAMES W. REED
Rutgers University,
New Brunswick

SHELAGH D. GRANT. *Sovereignty or Security? Government Policy in the Canadian North, 1936–1950*. Vancouver: University of British Columbia Press. 1988. Pp. xx, 385. \$29.95.

In the years before World War II, eleven million Canadians huddled close to the American border and turned away from the vast expanse of the Canadian

North, which seemed to have little value for a depressed nation. World War II changed the situation quickly as the United States, which had never acknowledged Canadian sovereignty in the Arctic, began to turn its attention northward and, from Alaska, westward. In peacetime, the Canadian government would not tolerate the United States building a road through the Canadian Northwest to Alaska, even though Prime Minister William Lyon Mackenzie King cherished his good relationship with President Franklin Roosevelt. When war came and, especially, when France fell and Canada was on the front lines, American defense interest in Canada was welcomed.

The United States was, of course, concerned about Japan and about the rickety state of Canada's Pacific defenses. Preoccupied with events in Europe, the Canadians allowed the United States to build the Alaska Highway, dozens of weather stations and air fields, and an expensive and quite useless oil pipeline. By 1943, there were far more American military personnel in the Northwest than there were Canadians, and American popular magazines were talking about the Canadian North as if it were a new American frontier. Canadian nationalists in the Department of External Affairs and their friends among the small Canadian intellectual elite reacted quickly, but it took a strongly worded memorandum from Malcolm MacDonald, the British High Commissioner to Ottawa, to alert Canadians to the dangers to their sovereignty in the North. Hugh Keenleyside of the Department of External Affairs is Shelagh D. Grant's hero, one who took a paternalistic approach to northern problems but who had a "vision" of a Canadian North that would be to the world of aircraft what the Atlantic Ocean had been to expanding Europe. It was also a vision that had a place for the indigenous peoples of the North and that recognized that educational and welfare spending were essential to right the numerous wrongs of the past.

The vision was not realized. Using American and Canadian documentary sources, Grant demonstrates that the Cold War and the fear that Canada would be the Belgium of World War III meant that the military would be preeminent in the Canadian North. The Canadians insisted on paying for the American installations built in wartime, but, after the war, they believed they could not afford the expensive installations that American and Canadian military officials deemed essential in the Cold War. Although the Canadians hesitated to make formal agreements and insisted on "civilian cover" for the extensive American activities, they let the initiatives of Keenleyside begin to die. The North had a military emphasis both for its inhabitants and for southerners.

Grant has written a most impressive work based on extensive research in governmental and private papers. She is careful to place the events in the North within the broader context of military, economic, and political history, and each chapter begins with an elegant, brief description of the larger setting. Grant

makes clear that the questions of national security and Arctic sovereignty have not yet been resolved. Those who seek such resolution in the future must read this important book.

JOHN ENGLISH
University of Waterloo

LATIN AMERICA

DOUGLAS V. ARMSTRONG. *The Old Village and the Great House: An Archaeological and Historical Examination of Drax Hall Plantation, St. Ann's Bay, Jamaica*. Assisted by ELIZABETH J. REITZ. (Blacks in the New World.) Urbana: University of Illinois Press. 1990. Pp. xvii, 400. \$44.95.

This elegantly produced volume can serve as a meticulous and valuable instructional handbook in the important new science of domestic archaeology in plantation contexts. Unfortunately, though, the theoretical problems discussed and impressive techniques deployed are not rewarded by conclusions, general or specific, that are either surprising or even conclusive.

Douglas V. Armstrong presents us with the fruit of several years of exhaustive fieldwork and laboratory analysis employing the latest methods, set in an admirable survey of the relevant literature and sources available, and a frank discussion of the problems he faced. The 227 pages of text are illustrated by numerous clear maps, diagrams, tables, drawings, prints, and photographs, supported by no fewer than fourteen appendixes and a comprehensive bibliography.

Armstrong quite rightly points out the need to build on the pioneering work of Jerome Handler and Frederick Lange, B. W. Higman, and others, to examine the archaeological evidence for the lifeways of the ordinary black laborers on West Indian plantations to supplement and contrast with the much better-known data on the white planter class. He is admirably aware of the need to relate archaeological material to written records, general histories, and, where feasible, oral traditions. He is also sensitive to the need to place each site examined in the general comparative context in order to assess its typicality and to place findings in perspective. The question remains, however, as to how far he has succeeded in his own case.

Armstrong is at pains to defend the typicality of the particular great house and slave quarters examined, but his description of the process of elimination that led to the selection of Drax Hall—one of the even fewer sites not still occupied, subsequently built over, or extensively plowed under, of the comparatively few Jamaican sugar estates having an extant series of records and maps throughout the period of its use—serves to argue that it was the site that chose the researcher rather than vice versa. He is scrupulous in matching where he can what was found in the ground

with the surviving estate records (not speculating too much beyond the evidence), and in discussing the unusual or unique features of the estate. But the question of typicality remains open until much more comparative spatial and temporal statistical analysis of Jamaican estates and their populations is completed, and (as Armstrong admits) many more laborers' villages are archaeologically investigated.

As already suggested, Armstrong's methodology is impeccable. Successive chapters describe in splendid detail the way in which the sites of the former village and great house were identified and uncovered and the artifacts analyzed, using the latest techniques. Special attention is paid to what can be determined about spatial relationships from structural remains in house-yard areas, about the quality and modes of life from the wide range of artifacts found, and of subsistence activities and dietary practices from the remains of food animals and food and medicinal plants. Most of these matters relate to the lives of the black laborers and their families in the Old Village, but a penultimate chapter makes a special attempt to compare and contrast the evidence recovered from the area of the lost great house in which the owners and their senior managers lived.

The interim findings, and the summary conclusions in the final chapter, are invariably well-supported and occasionally enlightening, but rarely unexpected and sometimes crushingly obvious. When making comparisons between the areas of black and white habitation, for example, Armstrong states: "It appears that Slave, Transitional and Free Labor-period samples are more similar to each other than they are to those associated with the Great House." Indeed, the prevailing disappointment of the book must reflect the author's own disappointment (however skillfully concealed) that he has been unable adequately to differentiate between the remains of the Drax Hall slave quarters and those of the subsequent "free" village. Whereas a resolute attempt is made to show that the two periods can be distinguished in the archaeological record, with even an intervening transitional period being delineated, Armstrong does not convincingly demonstrate a break or even very much change in the inhabitants' way of life. That material conditions did not much improve for ex-slaves is as much received wisdom as that "free wage labor" was a sham. But if the archaeological record cannot offer more precision than this about the nature of a crucial social change, it is not really adding much at all.

Such a conclusion, however, is not fair to this fine book. Armstrong himself argues for a more complete integration of documentary with archaeological and paleobotanical evidence, as well as many more case studies. In both respects, he has laid a methodological base that will be invaluable for subsequent scholars.

MICHAEL CRATON
University of Waterloo

DALE W. TOMICH. *Slavery in the Circuit of Sugar: Martinique and the World Economy, 1830–1848*. (Johns Hopkins Studies in Atlantic History and Culture.) Baltimore: Johns Hopkins University Press. 1990. Pp. xiv, 353. \$46.50.

In this unevenly, often circuitously written and repetitious work, Dale W. Tomich examines the nature of slavery and the plantation economy on the French Caribbean island colony of Martinique. Despite the time period indicated in the title, Tomich has reviewed the history of slavery in the Caribbean in general and the French Caribbean in particular, with an emphasis on the period after 1790. He blends archival sources gleaned in France and from the island with a good, although not exhaustive, survey of the secondary literature. Martinique, consonant with its limited geography and colonial affiliation, exhibited some minor variations on the regional pattern of sugar cane plantation systems in the Caribbean. The observations, therefore, are neither surprising nor novel. Sugar production in Martinique as elsewhere dictated the routine of daily life. Slaves were abusively exploited and overworked, and the system "was marked by contradictions, antagonisms, and conflicts throughout" (p. 281). What is attractively new in this study derives from Tomich's attempt to link the local organization and activity of Martinique to the competition with metropolitan beet sugar producers and the wider world sugar market. Tomich tries to see transformations in production and social reality as both interlinked and complex rather than simple, causal, or consequential. For example, the introduction of central factories forced a reorganization of cultivation and manufacture, but it also tended to erode the traditional social superiority of the plantation owners by eroding the distinction between sugar cane growing and other forms of agricultural activity.

This is not, however, an easily read or easily digested book. At times the work is merely impenetrably dense: "Neither the labor required to reproduce the slave nor surplus labor takes the form of value, and the commodity form does not relate the value of labor to the value produced by labor" (p. 132); "the social relations of slavery historically allowed the development of the social character of labor, the increase of the scale of production, and the expansion of the division of labor" (p. 198). At other times it is infuriatingly implausible: "the plant was re-covered with dirt, leaving a centimeter of the stem exposed" (p. 141); "One field was planted with mature cane ready to be harvested" (p. 143). Moreover, Tomich informs vastly more about the technical process of sugar production and the mechanical process of cultivation than the daily lives of the slaves or the nature of slavery. His work does not compare well with some established studies of Caribbean slavery. His work lacks the elegant clarity of Elsa Goveia's *Slave Society in the British Leeward Islands at the End of the Eighteenth Century* (1965), or the rigorous straight-

forwardness of B. W. Higman's *Slave Population and Economy in Jamaica 1807–1834* (1976), and the technical authority of Manuel Moreno Fraginals's *El Ingenio* (1978). It should, however, appeal to specialists with a sympathy for world-system analyses.

FRANKLIN W. KNIGHT
Johns Hopkins University

ANN M. WIGHTMAN. *Indigenous Migration and Social Change: The Forasteros of Cuzco, 1570–1720*. Durham, N.C.: Duke University Press. 1990. Pp. ix, 315.

Demographic studies of colonial Latin America largely have focused on the catastrophic decline of native American populations in the aftermath of the Spanish conquest and on regional patterns of population recovery or eventual extinction. This historiography grounds the conventional picture of colonial Indian societies, an image of endogamous communities of poor, conservative peasants, suspicious of outsiders, intent on preserving what few material resources remained within their control. This stereotype has gradually given way to a more complex and sophisticated understanding of the impact of Spanish rule as scholars have turned their attention to such topics as rural class formation, entrepreneurship among native elites, and voluntary participation in wage labor. Ann M. Wightman's book is the most comprehensive study to date of a demographic phenomenon whose significance has been widely recognized but little studied. Indigenous migration took place on a large scale in both New Spain and the Viceroyalty of Peru, but just how these population movements altered the social and political organization of rural towns and villages has been uncertain.

In this short but dense and ambitious book, Wightman explores the causes of indigenous migration, documents the size of the migrant or *forestero* population in Cuzco and its hinterland, looks at the integration of *foresteros* into existing communities and kin groups (*ayllus*), and studies how migrants transformed systems of production. She begins with the failure of Viceroy Francisco de Toledo's campaign in 1570 to congregate dispersed, mobile populations into concentrated villages, and ends with the devastating epidemic of 1720, when 80,000 souls died in the bishopric of Cuzco. At that time new legislation recognized the legal identity of migrants with land and officially granted them status equal to that of native-born community members. Wightman devotes fully one-third of her book to appendixes that offer statistical data on migrant occupations culled from labor contracts in the notarial archives, and demographic profiles of migrant populations derived from census reports. This material represents many hours of careful research and is an important part of Wightman's contribution to the literature, but it is not well-integrated into the text.

Finally, on a theoretical plane, Wightman chal-

lenges those who have argued that indigenous migration in colonial Peru was a measure of marginality and deculturation. She argues convincingly that migrants helped to resurrect native communities that otherwise would have died out, that *foresteros* preserved their ethnic identities despite moving into wage labor in the urban economy, and that migration itself was a form of resistance to colonial rule. All in all, her book deserves to be read by anyone interested in the social history of Latin America. Indigenous migration promises to command the attention of more and more scholars, and Wightman's book will be an essential starting point for those who follow.

KEVIN GOSNER
University of Arizona

JUNE E. HAHNER. *Emancipating the Female Sex: The Struggle for Women's Rights in Brazil, 1850–1940*. Durham, N.C.: Duke University Press. 1990. Pp. xvii, 301. Cloth \$42.50, paper \$16.95.

In this work, June E. Hahner aptly delineates the long roots of feminism in Brazil, demonstrating that contemporary movements have a historical pedigree worth learning about. In carrying out this task, she has strengthened the history of women and the social history of that country.

Because feminism had a nineteenth-century definition, Hahner begins with a survey of mid-nineteenth-century women. Due to a lack of historical monographs, much remains to be done in this period, but this work helps to fill some gaps with a thoughtful analysis of the ideas disseminated in women's magazines from the 1870s through the 1890s. As in most Latin American countries, feminism had its inception among urban, white, middle-class women, whose goals were to share the country's drive for improvement through education. Early demands for electoral rights were quashed by a strong male opposition and by division among the women themselves. Brazil abolished slavery in 1888 and feminism developed at the same time as the abolitionist campaign. The connection between the two movements was weak because women in Brazil did not have the same intellectual, legal, or social thrust of the North American suffragists and abolitionists.

The contrasts between poor and middle-class women and the relevance of ethnicity as a signifier of women's status and influence is suggested in the chapter dealing with women in the labor force. The difference in life styles and the ambivalent attitude of male leadership toward women workers were reflected in the lack of change in attitudes on gender relations in the early twentieth century, feminist discussions notwithstanding. The most important challenges to men's hegemony in public and private life began to take shape in the 1920s, a decade of deep intellectual and political stirring. At this point, the influence of North American and European wo-

men's suffrage movements stimulated the organization of Brazilian women for political rights. Female suffragism never caused much alarm in Brazil, possibly because its strong middle-class nature did not threaten social structures. Hahner follows the suffrage movement in detail in its international linkages and its intellectual and political development through 1940. She ends with a long epilogue assessing the legacy of feminism and the shaping of a second wave of feminists in the 1980s.

Women's movements in Brazil fared badly during the political crises in the 1890s, the 1930s, and the 1960s. The change in political climate in the mid-1980s, however, gives promise of a different outlook for women. The social needs of the nation, espoused by the recent feminists, seem to be making feminism more relevant to a larger audience, especially in the

cities. At this point a history of women's movements becomes an intellectual necessity for historians and for women themselves. Hahner has tried, whenever possible, to overcome the almost unavoidable leaning toward the political and the suffrage issue with her inquiry into other relevant issues. Yet the political endows this work with its internal dynamics, given the difficulties of sustaining an analysis of labor politics or the intellectual continuity of the process of female education. This is a sound, informative, and useful historical overview. It is based on women's writings, researched extensively in journals, newspapers, and public documents. It is a reliable tool for comparative studies of women's history and belongs in the library of all students of twentieth-century Brazil.

ASUNCIÓN LAVRIN
Howard University

Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed.

G. KASS-SIMON and PATRICIA FARNES, editors. *Women of Science: Righting the Record*. Assisted by DEBORAH NASH. Bloomington: Indiana University Press. 1990. Pp. xvi, 398. \$39.95.

G. KASS-SIMON, Introduction. CYNTHIA IRWIN-WILLIAMS, Women in the Field: The Role of Women in Archaeology before 1960. MICHELE L. ALDRICH, Women in Geology. PAMELA E. MACK, Straying from Their Orbits: Women in Astronomy in America. JUDY GREEN and JEANNE LADUKE, Contributors to American Mathematics: An Overview and Selection. MARTHA MOORE TRESKOTT, Women in the Intellectual Development of Engineering: A Study in Persistence and Systems Thought. L. M. JONES, Intellectual Contributions of Women to Physics. G. KASS-SIMON, Biology Is Destiny. PATRICIA FARNES, Women in Medical Science. JANE A. MILLER, Women in Chemistry. MAUREEN M. JULIAN, Women in Crystallography. PATRICIA FARNES, Afterword.

KATHLEEN D. MCCARTHY, editor. *Lady Bountiful Revisited: Women, Philanthropy, and Power*. New Brunswick, N.J.: Rutgers University Press. 1990. Pp. xiii, 201. Cloth \$35.00, paper \$14.00.

KATHLEEN D. MCCARTHY, Parallel Power Structures: Women and the Voluntary Sphere. ANNE FIROR SCOTT, Women's Voluntary Associations: From Charity to Reform. NANCY A. HEWITT, Charity or Mutual Aid? Two Perspectives on Latin Women's Philanthropy in Tampa, Florida. DARLENE CLARK HINE, "We Specialize in the Wholly Impossible": The Philanthropic Work of Black Women. KATHRYN KISH SKLAR, Who Funded Hull House? EDITH COUTURIER, "For the Greater Service of God": Opulent Foundations and Women's Philanthropy in Colonial Mexico. BRENDA MEEHAN-WATERS, From Contemplative Practice to Charitable Activity: Russian Women's Religious Communities and the Development of Charitable Work, 1861–1917. ALISA KLAUS, Women's Organizations and the Infant Health Movement in France and the United States, 1890–1920. ELLEN ROSS, Good and Bad Mothers: Lady Philanthropists and London Housewives before the First World War.

WOLFGANG J. MOMMSEN, editor. *Das Ende der Kolonialreiche: Dekolonisation und die Politik der Grossmächte*.

Frankfurt a.M.: Fischer Taschenbuch. 1990. Pp. 237. DM 18.80.

WOLFGANG J. MOMMSEN, Einleitung. RUDOLF V. ALBERTINI, Das Ende des Empire: Bemerkungen zur britischen Dekolonisation. JÜRGEN LÜTT, "Übertragung der Macht" oder "Sieg im Freiheitskampf"? Der Weg zur indischen Unabhängigkeit. BERNHARD DAHM, Der Dekolonisationsprozess Indoniens: Endogene und exogene Faktoren. DIETER BRÖTEL, Dekolonisierung des französischen Empire in Indochina: Metropolitane, periphere und internationale Faktoren. JÜRGEN OSTERHAMMEL, Die Chinesische Revolution als Prozess der Dekolonisierung. REINHARD SCHULZE, Ägypten 1936–1956: Die Nationalisierung eines kolonialen Staates. WILLIAM ROGER LOUIS, Die Vereinigten Staaten von Amerika und die Auflösung der europäischen Kolonialreiche: John Foster Dulles und die Suez-Krise des Jahres 1956.

JOHN A. LYNN, editor. *Tools of War: Instruments, Ideas, and Institutions of Warfare, 1445–1871*. Urbana: University of Illinois Press. 1990. Pp. xii, 262. \$24.95.

JOHN A. LYNN, The Pattern of Army Growth, 1445–1945. SIMON ADAMS, Tactics or Politics? "The Military Revolution" and the Hapsburg Hegemony, 1525–1648. WILLIAM MALTBY, Politics, Professionalism, and the Evolution of Sailing-Ship Tactics, 1650–1714. RICHARD HELLIE, Warfare, Changing Military Technology, and the Evolution of Muscovite Society. BRUCE P. LENMAN, The Transition to European Military Ascendancy in India, 1600–1800. DON HIGINBOTHAM, The Military Institutions of Colonial America: The Rhetoric and the Reality. JOHN A. LYNN, En avant! The Origins of the Revolutionary Attack. DENNIS SHOWALTER, Weapons and Ideas in the Prussian Army from Frederick the Great to Moltke the Elder. HEW STRACHAN, The British Army and "Modern" War: The Experience of the Peninsula and of the Crimea. MICHAEL HOWARD, Afterword: Tools of War, Concepts and Technology.

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RICHARD GRAHAM, editor. *The Idea of Race in Latin America, 1870–1940*. (Critical Reflections on Latin America Series.) Austin: University of Texas Press. 1990. Pp. 135. Cloth \$15.95, paper \$7.95.

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Other Books Received

The following books were recently received in the AHR office. Books listed here do not include works scheduled for review.

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- ACKERMAN, DIANE. *A Natural History of the Senses*. Paperback edition. New York: Vintage. 1991. Pp. xix, 331. \$11.00.
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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editor's discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

ARTICLES

TO THE EDITOR:

I wish to correct a possible misperception on the part of your readership.

In her interesting review of my book *Vienna and the Jews 1867–1938: A Cultural History* (AHR, 96 [April 1991]: 553), Marsha Rozenblit, among other things, criticizes me for performing “a tour de force of circular reasoning” in my handling of statistical data. She claims that I deduce from the fact that “65 percent of Jewish [*Gymnasium*] students had fathers in commerce or in the professions” the conclusion that “Jews formed the same percentage of the entire educated bourgeoisie.” If I had indeed claimed such a thing, then it would indeed have been extraordinary, not to mention “circular.” As a reading of my actual text will clearly show, however, I claim nothing of the kind.

What I do claim is that 65 percent of *Gymnasium* students with fathers in commerce, industry, finance, and the liberal professions were Jewish (a quite different proposition from that asserted by Rozenblit), and that this figure suggests that the large presence of Jews in Vienna's modern culture elite is explicable by a similarly large Jewish presence in the educated, liberal bourgeoisie of Vienna generally, as indicated by this figure from the *Gymnasien*. This extrapolation from the *Gymnasien* data to the cultural elite is indeed arguable, since it depends on an acceptance of the kind of approach to the social bases of Viennese culture and politics adopted by Carl Schorske on the one hand and John Boyer on the other. It is not, however, circular in the slightest but rather an attempt to put the social history of Vienna's Jews in the context of the city's cultural modernism, something not attempted by Marsha Rozenblit.

I have no explanation as to how Rozenblit could have made this mistake, but I feel it would be unfair to your readership for them to labor under this particular inaccuracy. Rozenblit's other major criticism of my book, her apparent denial of any place for the individual in the Jewish tradition, is similarly interesting but largely a matter of interpretation (albeit in my opinion wrong-headed). It does not involve a radical misrepresentation of my argument, as the above misreading of my statistical data does.

STEVEN BELLER

Alexandria, Virginia

MARSHA ROZENBLIT REPLIES:

There is no doubt that Steven Beller is making an important point when he argues that Jews dominated the cultural elite in Vienna because they formed a significant part of the educated liberal bourgeoisie in the city. My criticism, however, was not with his general point, or even really with his desire to use the Schorskean model to explain cultural productivity. I objected, and still object, to his use of the statistics to exaggerate the role of the Jews in the educated liberal bourgeoisie. His book clearly implies that Jews formed the overwhelming majority of that class. He bases this assertion on the fact that 65 percent of the *Gymnasium* students with fathers in commerce, industry, finance, and the free professions were Jewish. This statistic, however, merely reflects the general occupational distribution of the Jews of Vienna and does not tell us about the larger educated liberal middle class because it is not a representative sample. The fathers of Jewish *Gymnasium* students cannot be taken to represent all Jews, or indeed the educated liberal bourgeoisie, or even its Jewish sector. Most of the fathers of Jewish *Gymnasium* students were not themselves educated, thus they are not the group that Beller should discuss. As I said in my original review, all that the *Gymnasium* statistics prove is that 30 percent of the educated elite was Jewish, and that in itself is an amazing statistic in a city in which Jews formed only 8 percent of the total population.

As for Beller's insistence that concern for the individual was part of Jewish tradition, I must reiterate that such concern was not a part of traditional

Judaism, whose main focus was following the law because God had commanded it. It is true that under the influence of the Enlightenment, nineteenth-century Jewish apologists had emphasized individualism. Perhaps Beller should modify his argument in this direction.

Although Steven Beller and I disagree on many issues, I think that his book makes an important contribution to the scholarship on modern culture and on the Jews in Central Europe. He has raised some important questions and he has begun a lively debate.

MARSHA ROSENBLIT
University of Maryland,
College Park

TO THE EDITOR:

Congratulations on your issue on the Middle East! [December 1991] Its articles should be of great help to historians in understanding these important topics and presenting them to students.

I have a few caveats, intended to supplement, not denigrate, the achievement of your authors. First, although the preliminary editor's note suggests a historical rather than current-events perspective, the issue is framed so as to omit major areas whose current importance is not immediately obvious. Articles are dominated by the Arab countries from Egypt eastward, omitting North Africa, with a few articles on Iran and Israel. Although they cover the nineteenth century, there is no article on either the Ottoman Empire or Turkey. This skews the history of the area, including its Arab component, most of which was long a part of the Ottoman Empire and shared in its culture. It implies acceptance of one current Arab view of history that tends to downgrade the Ottomans as well as non-Arab minorities. Most articles are also limited to a single nation-state or area, slighting comparative considerations and regional perspectives.

These problems may be seen in the generally excellent article by Rashid Khalidi, in which he rightly notes the isolation of the study of Arab nationalism from broader historical trends and from comparative studies, and "a propensity toward compartmentalization along linguistic and national lines [which] . . . has led to an unfortunate situation in which those studying Arab and Turkish nationalism, for example, have often been unaware of the relevance of one another's work." Khalidi also notes the strong mutual influence of Middle Eastern nationalisms. In the rest of his article, however, he ignores his own insight and neither says how nearby nationalisms influenced Arab nationalism nor cites works on, for instance, Ottoman and Turkish nationalisms useful to the understanding of Arab nationalism.

Such works exist: the unjustly neglected masterpiece by Niyazi Berkes, *The Development of Secularism in Turkey* (1964) interprets the intellectual history of

the Ottoman Empire and Turkey since the eighteenth century. Its discussion of the different paths suggested by late Ottoman intellectuals—Pan-Islam, Pan-Turkism, and Ottomanism, and later Anatolian Turkish nationalism—has significant parallels to the discussions of Islam, Pan-Arabism, and Arab "nationalism in one country" ably covered by Khalidi. Political Pan-Islam was pioneered by Young Ottomans like Namik Kemal—a group well treated in Serif Mardin's *Genesis of Young Ottoman Thought* (1962). Only later was Pan-Islam picked up by the Iranian Jamal al-Din "al-Afghani" and by various Arab thinkers. Early Arab nationalism was in part a response to the Turkish nationalism of the "Young Turk" rulers and intellectuals before and during World War I, a nationalism discussed in David Kushner's *Rise of Turkish Nationalism, 1876–1908* (1977).

In a pre-World War I world in which members of the Egyptian ruling family had high posts in the Ottoman bureaucracy, and an Ottoman bureaucrat could become a hero of later Tunisian nationalists, it is misleading to speak of Arabs and Turks as if these words had the meaning then that they do now. As is known, the meaning of both words changed greatly in this century. Their change to a positive nationalist meaning is part of a revolution in ideas, not an unquestionable reality.

A different, more minor, consideration arises regarding Shaul Bakhash's excellent article on historical disputes concerning modern Iran. With his usual lucidity and intelligence, Bakhash has signaled the main debates, particularly in the interpretation of twentieth-century revolutions. I agree with his main criticism of my *Roots of Revolution*, that it does not sufficiently distinguish between pre and post-1973 economic developments. There is no excuse, as I had already published articles stressing the ill effects of Iran's handling of new oil income. My interpretation of the causes of the revolution has evolved, as seen in "Can Revolutions Be Predicted? Can Their Causes Be Understood?" *Contention*, 1 (Winter 1992).

In presenting the main contentious issues regarding modern Iranian history, Bakhash does not mention a number of significant works relevant to these issues. While he may have wished to stress the main players in major controversies, he could have had one long final note or appendix. As his article is the only one many people will read for bibliographic aid on modern Iran, this would have helped. In this spirit, I would suggest some other works useful to understanding twentieth-century Iran and its controversies; I realize that some of these are too recent to have been cited by Bakhash.

Bakhash's own outstanding book, *The Reign of the Ayatollahs* (1984) deserves mention. Regarding the role of the ulama in the constitutional revolution, Vanessa Martin, *Islam and Modernism: The Iranian Revolution of 1906* (1989), and now Mangol Bayat, *Iran's First Revolution: Shi'ism and the Constitutional Revolution of 1905–1909* (1991), present major new

research and conclusions. On the religious background of the revolution, Shahrough Akhavi, *Religion and Politics in Contemporary Iran* (1980), and Michael Fischer, *Iran: From Religious Dispute to Revolution* (1980), have important material. One could also mention books and articles by some of the following: Hossein Bashiriyeh, Mohsen Milani, Ali Banuazizi and Ahmad Ashraf, Farhad Kazemi, Hamid Enayat, Abbas Amanat, Janet Afary, Val Moghadam, Farrokh Moshiri, Mehran Kamrava, Houchang Chehabi, Hamid Dabashi, Ali Sheikholeslami, Farideh Farhi, Roy Mottahedeh, Sami Zubaida, Misagh Parsa, and others—not to mention writers on Iranian international relations (omitted here as I have emphasized topics stressed by Bakhash).

At the end of his article, Bakhash mentions areas that have remained almost unstudied, including regions outside Tehran, popular political culture, workers, and women. He is right on all, but on women and the provinces in the revolution there are fine works by anthropologists Mary Hegland and Erika Friedl. Hegland's studies center on a large village near Shiraz before and during the revolution; covering women in the revolution in "Political Roles of Aliabad Women: The Public-Private Dichotomy Transcended," in *Women in Middle Eastern History: Shifting Boundaries in Sex and Gender*, Nikki R. Keddie and Beth Baron, eds. (1992). Other works by anthropologists who were in Iran near the time of the revolution are also relevant.

A major work on popular culture is Paul Vieille and Farhad Khosrokhavar, *Le discours populaire de la révolution iranienne* (1990). This is useful not for its bizarre rejection of history but for its numerous insightful interviews of popular-class Iranians. On workers, a study about the immediate postrevolutionary period that tells much about workers' ideology is Assef Bayat, *Workers and Revolution in Iran: A Third World Experience of Workers' Control* (1987). There are several works on women and the revolution, by Afsaneh Najmabadi (Azar Tabari) and others.

On most of the needed research mentioned by Bakhash, new information must come mainly from work in Iran, which a few Americans have recently been able to do. Their number should grow before long. Also, there has been research by Japanese and Europeans, and though problems remain, there is reason to believe that things will open up further, thus enabling scholars to deal with some of the issues signaled by Bakhash and with others.

NIKKI KEDDIE
University of California,
Los Angeles

RASHID KHALIDI REPLIES:

The briefest of replies to Nikki Keddie's letter is in order, if only to agree fully with her emphasis on the importance of the Ottoman Arab context for a fuller understanding of modern Arab history, and on the significance of the interaction between Turkish and

Arab nationalism. I did not refer to the many important works on these subjects noted by Keddie because of the limits that were set to my topic by the editors, and because I mistakenly assumed that they would be dealt with in other articles in the December 1991 issue of the *AHR* devoted to the Middle East. In referring to al-Afghani in my article, I might also have added that the standard biography was written by Nikki Keddie herself: *Sayyid Jamal al-Din "al-Afghani": A Political Biography* (1972).

RASHID KHALIDI
University of Chicago

SHAUL BAKHASH REPLIES:

There is certainly a great deal of other worthwhile material, for example the work of Mary Hegland, that might have been covered in an article on Iran. But, as Nikki Keddie surely realizes, there are always limitations of space, and some selectivity is inevitable. It serves little purpose merely to list a lot of authors, even in "a long final note or appendix," and to note that their work is "significant," when the whole intention of the essay was to discuss in some depth controversies among historians of modern Iran and the manner in which different authors have approached these issues.

SHAUL BAKHASH
George Mason University

FILM REVIEWS

TO THE EDITOR:

Numerous reviewers, such as Ellen Carol DuBois ("Film Reviews," *AHR*, 96 [October 1991]: 1140), have perceptively pointed out the strengths and weaknesses of Ken Burns's *Civil War* and indeed of the entire genre of documentary historical film.

But one shortcoming seems apparent in almost all such ventures and has not been addressed so far as I know. I refer to the practice of using a photograph or even a film clip of a "look-alike" for a particular event or artifact. In *The Civil War*, to take an area in which I have some competence, the narrator speaks of the *C.S.S. Virginia*, but the photograph shown is that of the much later ironclad ram *C.S.S. Stonewall*. Further on, the narrator talks of the *U.S.S. Monitor*, but the photo shown is that of a monitor. I am sure other sharp-eyed historians could provide further examples from this and other historical documentaries.

This is not antiquarian nit-picking. Historians know that the most seemingly insignificant fact might be pregnant with historical meaning. And even in and of itself that fact must not be tampered with; it might prove essential for our historical constructs and theories.

To take a homey example: a contemporary British

documentary on the 1940 Blitz very briefly shows an unlucky hitchhiking Englishman making a rude finger gesture to a passing motorist. This little scene, if authentic, could help a researcher in popular culture to state with some certitude that the gesture was current at least by 1940. But what if that clip was of a later bombing and had simply been inserted into the documentary to illustrate conditions in the Blitz? ("If you've seen one blitz, you've seen 'em all.")

On a more sober note, one does not have to be a Kennedy conspiracy buff to value each frame of the Zapruder home movie film. And I have certainly had enough of model aircraft diving over "Pearl Harbor," or the "Storming of the Winter Palace" as reenacted by Sergei Eisenstein but not so credited.

Although I have not seen my point raised in print by historians, I have had it confirmed in conversations with colleagues. It is past time for us to lift our voices against this anti-historical device. (The opinions expressed here are, of course, my own, and do not necessarily represent those of the U.S. Army or the U.S. Army John F. Kennedy Special Warfare Center and School.)

STANLEY SANDLER
U.S. Army John F. Kennedy
Special Warfare Center and School,
Fort Bragg, North Carolina

REVIEWS OF BOOKS

TO THE EDITOR:

In his review of my book, *The Arrogance of Faith: Christianity and Race in America from the Colonial Era to the Twentieth Century* (AHR, 96 [October 1991]: 1281), Gaines M. Foster commits the cardinal sin of book reviewers: he accuses me of saying something I did not say, then criticizes me for saying it. After noting that I found the Bible to be inherently racist, he contends that I undermined my own thesis by arguing that defenders of slavery misinterpreted the Bible, which they presumably would not have to do. I said no such thing. I did say that slaveholders, like everyone else, interpreted the Bible to suit themselves; and, when southern religious leaders rationalized specific biblical lessons like the Golden Rule, they were, to those who had used those lessons to attack slavery, misinterpreting the Bible. Foster's argument is a sophism that implies that I believe the Bible says only one thing and that it has only one correct interpretation. In fact, I used hundreds of biblical references, from Genesis to Revelation, in both text and notes. Two chapters deal exclusively with the Bible's language, mythology, and countless contradictions—and their use by people who enslaved Africans and exterminated Indians. Parts of other chapters discuss the racism inherent in such biblical ideas as the concept of a Chosen People and the claim of

"dominion over the earth," ideas whose applications led to the oppression and annihilation of millions of people whose only crime was not having white skins.

Foster also complained because I did not use "unpublished letters or diaries, or local church records." My book deals with fundamental ideologies and practices that evolved over centuries. It is not a case study of a handful of people who lived in one area for two or three decades. Rather, I focused on the beliefs and conduct of men of influence—kings, presidents, church officials, religious activists, legislators, landholders—the movers and shakers who changed the course of human events. Poring over unpublished manuscript collections would have turned up a great deal of insignificant information about a few ordinary people. I was concerned with a few significant things about a great many extraordinary people.

Finally, Foster implies, in a pejorative reference to "a brief closing chapter," that my book is uneven. The *Arrogance of Faith* is divided into nine chapters that average 42.7 pages (excluding notes). The first six chapters are topical and run from 33 to 48 pages. The last three chapters are chronological examinations of churches and racism in the colonial period (43 pages), the antebellum era (51 pages), and the aftermath of slavery from the Civil War to World War I (46 pages). Why Foster chose to derogate my discussion of a critical period with an incorrect (and irrelevant) reference to chapter length escapes me completely.

FORREST G. WOOD
California State University,
Bakersfield

Gaines M. Foster does not wish to reply.

THE EDITOR

TO THE EDITOR:

I was stunned to learn from John Ferling's review of Willard Sterne Randall's *Benedict Arnold: Patriot and Traitor* [AHR, 96 (October 1991): 1289] that Randall's "ability to simplify complex military engagements is a sheer joy."

The first half of this 667-page book (pages 84 to 368, to be exact) appears to be a detailed and accurate account of military actions in northern New York and Canada in 1775, 1776, and 1777, when in fact this half of the book is replete with errors of historical fact and interpretation, and both scholars and general readers are seriously misinformed about the military history. A number of fictitious scenes are created and presented as historical occurrences.

The numerous factual errors can be classified under the following six broad categories:

1. American and British military units are repeatedly misidentified, and a number of unfortunate people suffer the same fate.
2. The strength and casualty figures of both the American and British armies are generally consider-

ably off-base, although correct data on these figures, based on original returns, are readily available.

3. Six American generals and officers are unfairly traduced, perhaps to make Arnold "look better."

4. The narrative of the 1777 Burgoyne campaign contains an amazing number of serious geographical errors (forts and posts mislocated), many of which could have been avoided by consulting a modern road map.

5. Arnold is credited with making several critical military decisions in the 1776 and 1777 campaigns with which he had little or absolutely nothing to do.

6. The account of the military actions and events of the 1777 Burgoyne campaign is horrendously erroneous, as the following episodes illustrate. *Scene 1*: Randall informs us that Burgoyne's army came "rattling down the west shore of Lake Champlain on a newly built corduroy road of logs," when in fact he transported his army south on Lake Champlain in 400 bateaux. *Scene 2*: Randall reports that the American army fled west and south from Ticonderoga "down Wood Creek and along the west shore of Lake George," a line of retreat that is geographically impossible: in fact, the main body marched east and then south through Vermont. *Scene 3*: Randall provides an inaccurate and incomplete account of the Battle of Hubbardton, Vermont, July 7, and forgets to mention the Battle of Fort Ann, New York, July 8. *Scene 4*: Randall informs us that on July 24 Arnold "came up with an ingenious expedient" to slow the advance of Burgoyne's army. He sent out hundreds of axmen into the forest to fell trees so as to block the roads. Unfortunately for this scenario, Major General Philip Schuyler had issued these very orders 15 days before Arnold arrived in camp. *Scenes 5 and 6*: Randall presents incomplete and inaccurate accounts and data on the Battles of Oriskany, New York, August 6, and of Bennington (fought in New York), August 16, 1777. His data on the state of Fort Stanwix, New York, and the composition of its garrison in August 1777 are also incorrect. *Scenes 7 and 8*: Randall outdoes himself in invention and misinformation when he reaches the climax of the Burgoyne campaign—the two great Battles of Saratoga, fought on September 19 and October 7 at Bemis Heights. He presents a completely inaccurate account of the advance of the American army north up the Hudson River to Bemis Heights (now Saratoga National Historical Park), August 20 to September 12. This is followed by an equally inaccurate account of the fortifying of the American camp, September 12 to 24. Undeterred by facts, Randall next has Arnold inaccurately deploy the American army in line of battle on September 12 for Major General Horatio Gates, his commanding officer! Contrary to the author's imagination, the actual deployment of troops was based on orders that Gates issued on September 5.

Finally, Randall's apparently detailed accounts of the two Battles of Saratoga are documented by a total of only nine footnotes, and they support only re-

marks that are said to have been made by Arnold or others during the battles. The many American, British, and German diaries, journals, and letters of soldiers involved in these actions have not been consulted, and the result is largely fiction.

Randall's narrative of 1775–1777 military events is well written and interesting, but is it history or a historical novel? In my opinion, at least the first half of this book falls into the latter category.

CHARLES W. SNELL

Retired National Park Service Historian

WILLARD RANDALL REPLIES:

Charles Snell's categorical attack on my book, *Benedict Arnold: Patriot and Traitor*, asserts numerous and major errors both in fact and interpretation and further alleges the use of fiction and fictional scene-setting. Fortunately, Snell is wrong on every count. It is always difficult to know where to begin to unravel such a Gordian knot of angry invective, amateurish antiquarianism and downright unwillingness to learn anything new about history, especially when such prejudices are based on careless reading and antediluvian source materials. But let me try.

First, I have written a book Snell would not have written in a way he would not have written it. Snell obviously prefers compendious, day-by-day, blow-by-blow military history, especially of the Northern campaigns of 1775–77. I wrote a biography of Benedict Arnold and felt compelled to follow only his actions and words and the places where his life intersected events and their pertinent backgrounds, which is always a question of the author's judgment. Snell wants complete histories of battles and garrisons and orders written at Oriskany, Ft. Stanwix, Fort Ann, and Hubbardton. For a complete modern history of the engagement of the American rearguard at Hubbardton in July, 1777, for instance, I refer him to John Williams (Colonel, U.S.A. Retired) and his wonderful little book, *The Battle of Hubbardton*. Williams spent years researching this action. The result is admirable, and Col. Williams has both privately and publicly upheld my research on the Northern Campaign of 1777 and in fact was present when I was called upon by the Vermont Division of Historic Preservation to dedicate its new interpretive center on the Hubbardton battlefield. But Benedict Arnold had no connection with Hubbardton, so my depiction was terse and only by way of background for later events.

Yet I made a considerable effort to straighten out the paths of military roads in the Northern campaigns, something that cannot be done with a modern road map. In fact, I made use, in addition to contemporary maps and accounts, of a University of Vermont aerial infrared photographic study to determine the exact route of the Crown Point Military Road. It is true that the bulk of Burgoyne's army floated south on bateaux, but once Lake Champlain

narrowed at Crown Point, General Phillip's heavy artillery was brought down the remaining distance along a newly constructed log road, and things rattled.

There is a second major difference in how I wrote this book from how, I believe, Snell would write his. That has to do with my system of annotation, which I thought was self-evident but I am not sure Snell grasped. He no doubt would adhere to the Chicago *Manual of Style*, regardless of the heft or cost of the resulting volume or volumes, but I did not have the federal budget to subsidize my work. In each of my books, I have used a telegraphic system of notation, as have many other scholars. Numbered endnotes identify only quotations, figures, etc., which are drawn from primary sources. Preceding the numbered notes to each chapter, I cluster a selection of background subjects, each with sources that were found to be especially valuable and pertinent. Full particulars of each source together with other works that were read and drawn upon significantly are listed in the selected 580-source bibliography and the accompanying list of archival sources and journals that were consulted during five years of research.

Thus I did far more research on the two Battles of Saratoga than the nine notes Snell alludes to, including a full year of research on Thaddeus Kosciuszko, including his designing of the American works, for a PBS documentary aired during the Bicentennial, and I have been working over this material during the intervening fifteen years.

As for fictionalizing, none was intended or done. But if a writer does not share Snell's view of half a dozen unspecified generals, he writes fiction. And what are some of the "amazing number of serious geographical errors?" I can only guess he means that the tiny Fort Gage was on the *northern* shore of Lake George. If there were a few minor errors that could not be caught in the Morrow hardcover edition, they have already been corrected in the Quill paperback.

But who can correct Snell's lack of comprehension of strategy and military procedure? For more than a year before Saratoga, Schuyler had been listening to Arnold's ideas about imposing delay on the British. As a divisional commander at Freeman's Farm, Arnold actually did deploy troops, once the battle started.

Snell can read all the journals and diaries he wants, but can he evaluate what he has read or understand it in the light of new research he has overlooked? When he writes and publishes his version of the Northern campaigns, no doubt there will be a sharpshooter or two in the high grass waiting for him.

WILLARD STERNE RANDALL
University of Vermont

JOHN FERLING REPLIES:

What should a potential reader expect in a 700-word book review? The reader is best served, I think, by a review that judges the overall merits of the book under consideration. The reader must be informed if the book is seriously flawed, whether by major inaccuracies, insupportable conclusions, illogical organization, or dull and turgid writing. The reader of a short review certainly deserves to know whether the book breaks new ground and how it relates to other literature on the topic. Finally, the reader should be apprised of the author's objectives, as well as whether these aims were realized. The object of a brief review should not be the compilation of inconsequential errors.

The principal flaw in Willard S. Randall's *Benedict Arnold: Patriot and Traitor*, as I indicated in the review that appeared in the *AHR* (October 1991), is that the study devotes little attention to the subject's life following his treasonous conduct in 1779. With the exception of that shortcoming, however, Willard Randall has produced not merely a fine study of Arnold, but the best biography of this complex man that is now available.

Randall has done what the life writer must do. He has produced a well-organized and engagingly written account of Arnold's life. Moreover, he has succeeded in getting behind what Leon Edel once called the "mask of life" of his subject, so that his readers are provided insights into the driving forces behind this embittered and aggressive man. I continue to believe that readers of *Benedict Arnold* will discover a sound, judicious, and enlightening account of Randall's enigmatic subject.

JOHN FERLING
West Georgia College

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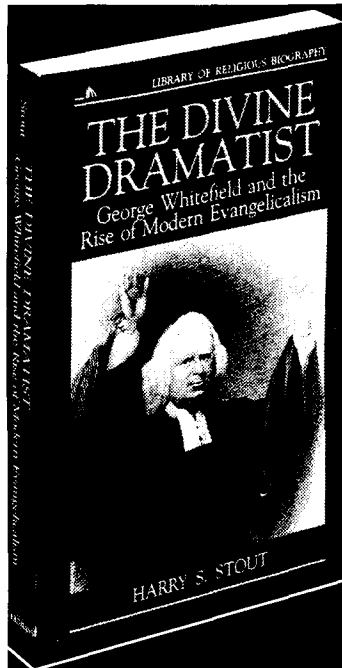
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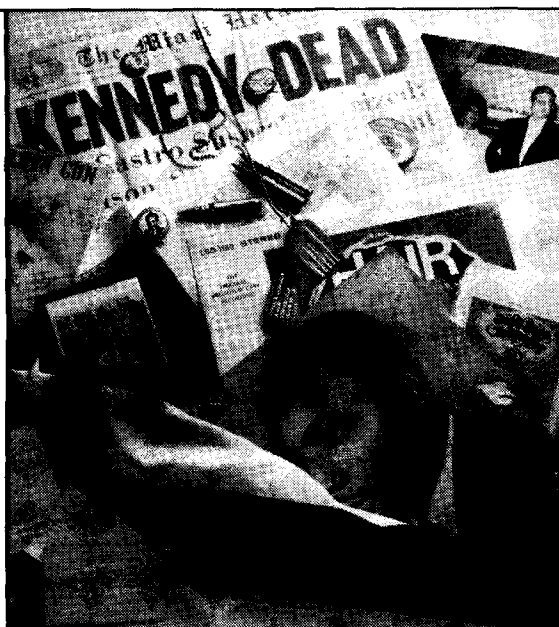
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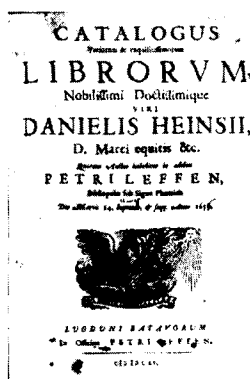
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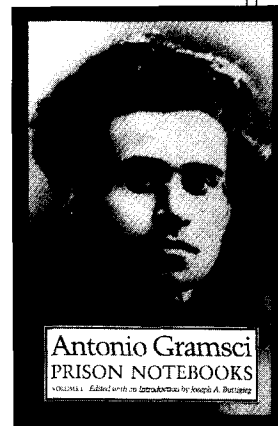
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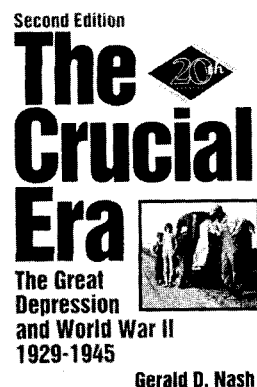
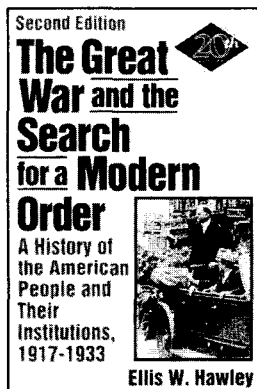
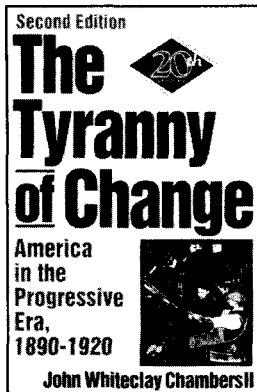
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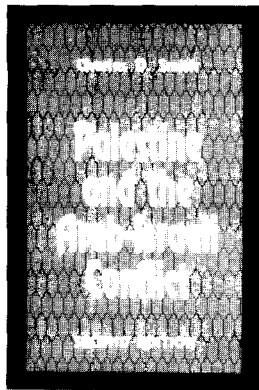
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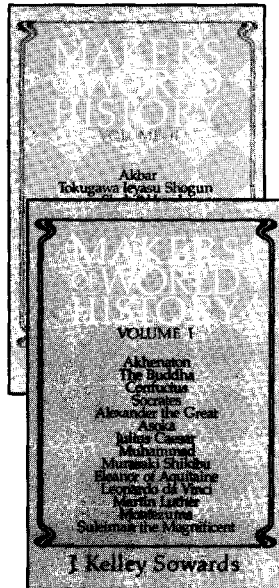
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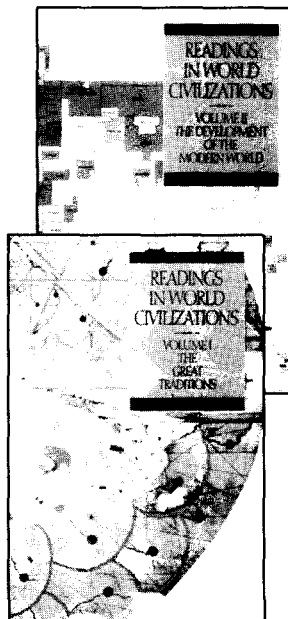
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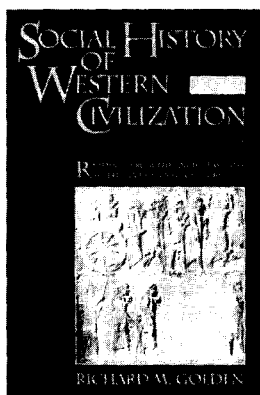
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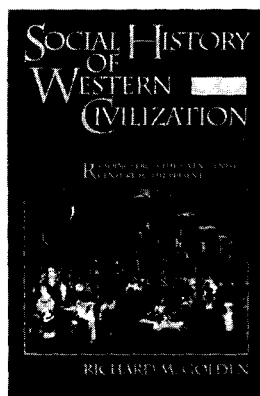
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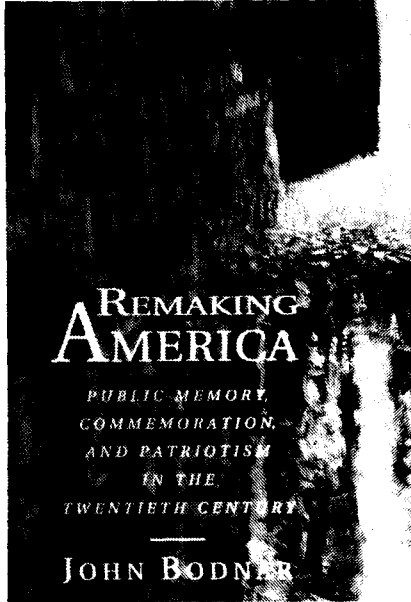
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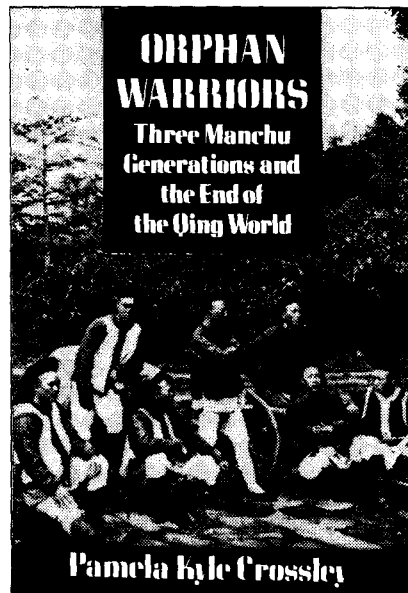
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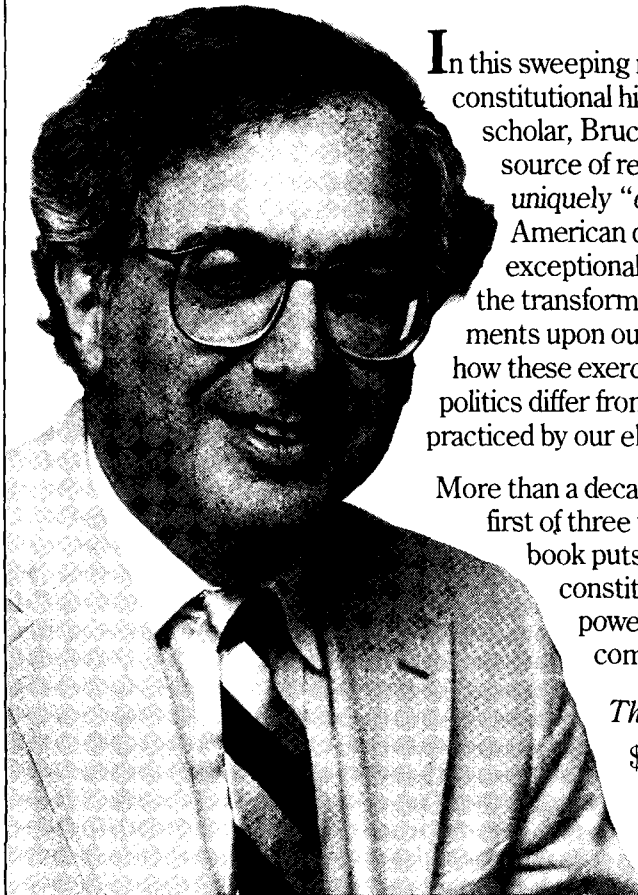


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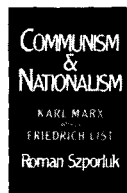
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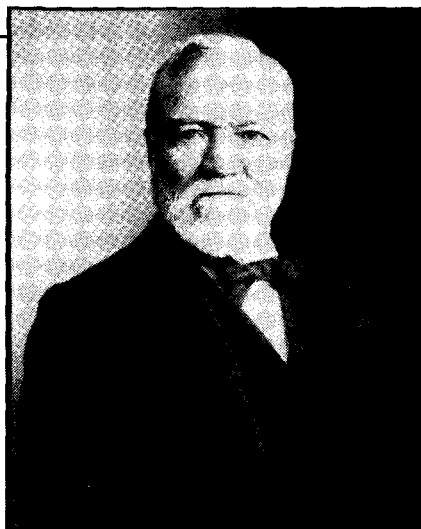
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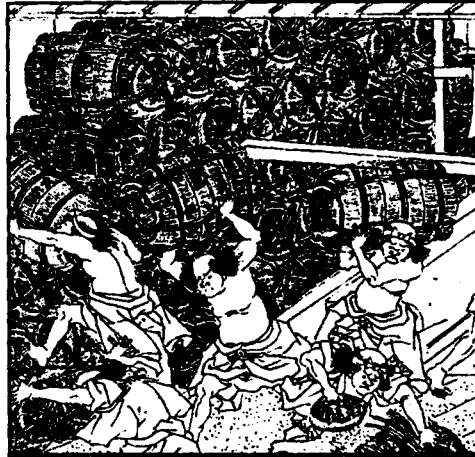
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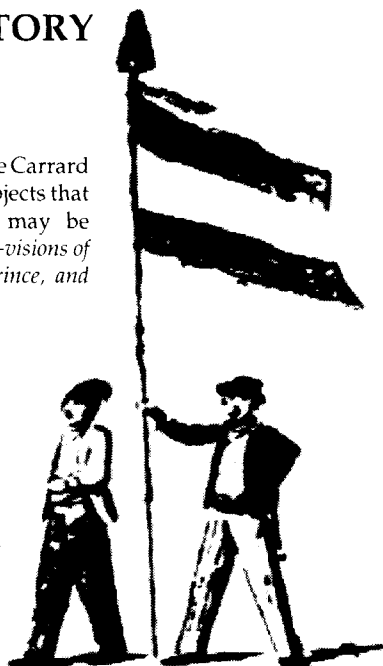
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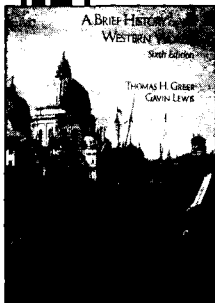
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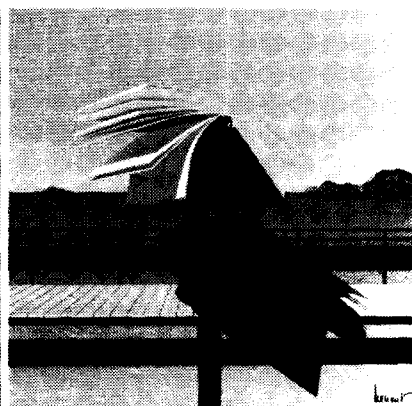
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